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To the Torquay Institution
presented by the
author

MISCELLANIES:

BEING A COLLECTION OF

MEMOIRS AND ESSAYS

ON

SCIENTIFIC AND LITERARY SUBJECTS,

PUBLISHED AT VARIOUS TIMES,

BY

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VOL. I.

EXPERIMENTAL AND GEOLOGICAL MEMOIRS.

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TO THE

PRESIDENT AND FELLOWS OF MAGDALEN COLLEGE, OXFORD,

This Collection of Memoirs and Essays,

WHICH OWES ITS EXISTENCE IN GREAT MEASURE TO THE

ADVANTAGES DERIVED FROM A LIFE-LONG CONNEXION

WITH THAT SOCIETY,

IS INSCRIBED, WITH THE KIND REGARDS AND

GRATEFUL ACKNOWLEDGMENTS OF

THE AUTHOR.

PREFACE.

THIS Volume of Miscellanies owes its origin to a desire on the part of its Author to rescue from a premature oblivion a number of Memoirs and Essays, spread over a series of years, and either merged in the Transactions of learned Societies, Scientific Periodicals, and the like, or else brought out originally as detached Pamphlets, in which form the memory of them is soon lost, when the occasion which called them forth has passed away.

Its contents, together with those of my previous publications[a], may therefore be regarded as some of the fruits of a life chiefly spent in tranquil intellectual occupation, under the fostering wing of one of those great semi-monastic establishments which are peculiar to this country; and, however slight their intrinsic value, considered as contributions to the stock of human knowledge, may be, they will serve at least to shew, by their number and variety, what might be accomplished by persons gifted with greater energy and more profound attainments, through the aid of Foundations, in which an exemption from domestic cares, and a liberal provision for all the reasonable wants of a celibate life, afford such facilities for the indulgence either of literary or of scientific tastes.

The Work is divided into four parts; the first, reporting the results of experimental inquiries relating to Vegetable

[a] "Description of Active and Extinct Volcanos." 2nd Edition, 1848.
"Introduction to the Atomic Theory," 1852.
"Lectures on Roman Husbandry," 1857.
"Lectures on Climate," 1863.
"Essay on the Trees and Shrubs of the Antients," 1865.

Physiology, Agricultural Chemistry, and the like, which have occupied my attention at various times; the second, consisting either of observations of my own, or comments upon those of others, having reference chiefly to Volcanic, and other allied phenomena; the third, comprehending Essays on various topics relating to Natural History and Physical Science; and the fourth embracing Disquisitions on subjects of a Literary description.

The first part begins with "Researches on the Influence exerted by Light upon the functions of Plants," by which, it is believed, two points were first established, namely, the description of solar rays which act most efficiently upon the green parts of plants; and the fact, that nitrogen, as well as oxygen gas, is evolved by them during the continuance of sun-light.

My Memoir on this subject has been so long published, that its contents seem forgotten, and hence Draper and others, who arrived at the same conclusions subsequently, have had the credit of their discovery assigned to them[b].

The Paper on Ozone may be regarded in some degree as a complement to the former, as it points out another function discharged by the leaves, connected with the same great end of purifying the atmosphere, by removing the impurities engendered in it by the processes of animal life and decay, through the medium of that principle, which seems to be generated by the leaves of plants during sunshine, and which removes by oxidation any such noxious products.

It is hoped, that the scepticism which exists in some quarters as to the identity of the principle in the atmosphere which changes the colour of iodised paper, with Ozone, will be dispelled by the late Observations of Dr. Andrews, of Belfast, communicated to the Royal Society in June, 1867.

Other Papers, On the Influence exerted by Light upon Germination, On the Vitality of Seeds, and On the Power

[b] As by Dr. Forbes Winslow in his recent work "On the Influence of Light upon Animals."

which certain Classes of Plants possess of living in an Atmosphere charged with a larger proportion of Carbonic Acid than now exists in the Atmosphere, complete this part of the series.

The next set of subjects connected with Vegetable Physiology which is entered upon, relates to the functions of the Roots, which are shewn to be, not merely passive instruments, destined to transmit fluid from the soil to the upper parts of the plant, but, so long as their vitality continues unimpaired, to possess some active power of selecting the materials best suited for the nutrition of vegetables.

This same selective faculty is shewn, indeed, to be possessed in common by all parts of the vegetable organism, and, hence it happens, that whilst *abnormal* ingredients are rejected altogether, *normal* ones appear only to be secreted in the proportions most suitable to the organs which absorb them.

These results, which were in part arrived at previously to the year 1833, were more fully established by the experiments carried on at a later period, of which an account is given in this volume, and their correctness appears corroborated by the researches of Liebig, Way, and others.

The above inquiries, not less than the former ones, On the Influence of Light, point to the conclusion, that plants are something more than mere chemical laboratories for the conversion of dead matter into food for the animal creation, and also that they bring about the changes which take place within their tissues, through the operation of a vital principle, upon which light and heat act as stimuli, as they do upon the nervous system of animals.

The next set of researches noticed in the volume has reference chiefly to Agricultural Chemistry, but as several of these possessed only a temporary interest, and as the one best worthy of insertion, namely, that "On the Rotation of Crops," selected as the Bakerian Lecture for 1843, is voluminous, it has been thought sufficient to reprint a *resumé* of the results arrived at in it, pointing out their disagree-

ment with the Theory of the elder Decandolle as to the poisonous nature, with reference to plants of the same or of allied species, which he attributed to the root excretions, and also the distinction which they tend to set up between the active and dormant condition belonging to the mineral ingredients present in a soil.

This Paper is followed by one suggesting a method of determining whether a rock which appears destitute of organic remains at present could ever have contained them; and, although fully sensible that the examination of soils and rocks must be pushed much further before any general conclusions can be safely deduced, I am at a loss to perceive any flaw in the principle upon which the inquiry proceeded.

In a short Paper, which concludes this portion of the volume, the presence in bones of Fluoric acid, a fact which had been previously disputed, is shewn to hold good generally, although the difference between its amount in fossil and in recent ones still remains a problem, no less mysterious than the reason, why two principles so little allied in their properties, as Fluorine and Phosphorus, should be found so constantly associated in nature, as I have pointed out in my Paper to be the case.

The next part of this collection consists of Memoirs relating to Geology, which, if it had included all that I had at various times published on that subject, would have swollen to very considerable dimensions.

As, however, the substance of the greater number of them has been already embodied in my general work on Volcanos, I have admitted into this collection only a few which record some fact possessing a special interest, or some general bearing upon this class of phenomena.

Thus I have, in the first Memoir, noticed a remarkable case of adhesive affinity shewn in the disengagement of large volumes of aqueous vapour, of sal ammoniac, and of

free muriatic acid, from the cells and cavities of a current of lava already partially cooled; a fact in accordance with that of the Occlusion of Hydrogen Gas by meteoric iron, announced by the Master of the Mint in a late Paper, and adding one more to the points of resemblance between aërolites, and the contents of the earth's interior.

The second Memoir records a new phase in the Volcanic operations going on at Vesuvius, namely, the evolution of Marsh-gas and of Naphtha, along with Carbonic Acid, accompanied with an elevation of the coast for several miles near Torre del Greco in the year 1861.

The third Memoir advocates views which, I fear, are not likely to find support amongst the present race of Geologists, who are in general prepossessed in favour of an opposite theory.

But this very circumstance renders it the more important to place before them certain phenomena, which seem not easily resolvable upon their principle of slow and gradual elevation, and which might lead us to suspect, that in the physical, as well as in the moral and political world, slow changes carried on for a long series of years, are liable to be succeeded from time to time by some sudden outburst, accomplishing in a day more than appeared to have been brought about in a century before, although, perhaps, the latter may have been inaugurated by the internal movements that preceded it.

If the slow decay of the ancient religious beliefs which had been going on stealthily for centuries, paved the way for the sudden introduction of Christianity, or if the gradual spread of knowledge and increase of power amongst the *Tiers Etat*, were followed by that violent upheaving of the lower ranks of Society which took place at the Epoch of the great French Revolution, it might seem consistent with analogy to suppose, that in the Physical World also, slight movements of long continuance are followed by convulsions of greater intensity but of limited duration.

At all events, so long as it is assumed, that paroxysmal action constitutes a part of Nature's economy,—and Sir Charles Lyell himself, in the last Edition of the "Principles of Geology," does not altogether deny this position,—we seem justified in expecting that Volcanic rocks, of all others, would be the ones most liable to be so affected.

The fourth Memoir proposes a mode of accounting for the existence of Ammonia amongst the products of Volcanic action.

Although it is conceivable that its formation in the interior of the earth may be the result of the immense pressure there exercised, by which that direct union of its constituents, Hydrogen and Nitrogen, may be occasioned, which cannot be brought about at the surface, yet the discovery made of the strong affinity which subsists between Nitrogen and certain metals, and which gives rise to a class of combinations, called Nitrides, decomposable by the fixed alkalies, and thus generating Ammonia, seems to furnish a sounder basis for speculation, and, accordingly, I have availed myself of this fact, in order to explain the origin of the Ammonia so frequently evolved at Vesuvius and elsewhere.

Of the Memoirs relating to particular Volcanos, the substance has been already embodied in my general work on this subject, but I have made room for a Lecture on the Antiquity of those in Auvergne, both in order to expose the error of imagining the igneous action displayed in that country to have extended down to historical times, and also as giving me an opportunity of reviving the recollections of my younger days, by contrasting the merits of two early instructors in Geology, namely, Professors Jameson and Buckland[c], who for many years stood foremost amongst the cultivators of that Science in North and South Britain.

Of many Memoirs published by me at various times on

[c] For a lively but rather sarcastic picture of his manner of Lecturing, see a little Poem by the late Bishop Shuttleworth, included in a series of Fugitive Poems, &c., just published by Messrs. Parker.

the subject of Thermal Springs, I have only made room for one, that on the Bath Waters, in which I have placed upon record the results of observations made by me daily for a month in 1833, to ascertain quantitatively and qualitatively the Gases there given out from the interior of the earth, and have also pointed out the confirmation which the latter seem to afford to the Chemical Theory of Volcanos.

For although the latter is dwelt upon at length in my Work on "Volcanos," I have permitted myself to allude to it here again, inasmuch as various facts corroborative of the view I had taken, have been subsequently brought to light. One of them, I may add, namely, the evolution of Hydrogen from a Crater, has been only the other day borne out by the application of the new method of Spectrum Analysis to the case of the flames which issued from the new Volcanic vent at Santorino, as has been done by M. Janssen, despatched by the Minister of Public Instruction at Paris to investigate the phenomena there exhibited.

The account of the Phosphorite Rock of Spanish Estremadura closes this portion of the volume. It was the fruit of an expedition undertaken by myself and Captain Widdrington, R.N., to that locality, at the instance of the Royal Agricultural Society, the members of which were anxious of obtaining a ready and abundant supply of mineral phosphate, to replace the deficiency of bones for fertilizing their land, which had already begun to be felt. And although the want of cheap conveyance, either by land or water, at that time appeared to render the journey barren of results, I am happy to find that our labours were not thrown away, as the new railroad connecting Madrid and Lisbon comes so near to the place, that it will be quite possible henceforth to transport the material to the coast at a very moderate outlay.

The third portion of the volume contains a selection of Essays published by me at various times on subjects connected with Science and Natural History.

I have placed at the head of these a few relating to Vegetable Physiology, as for instance, A Sketch of the Philosophical Character of the elder Decandolle, whose writings contain the germ of many of the speculations which have so much occupied botanists since his epoch.

Next in order is an attempt to trace the origin of Epidemics to some of those Lower Organisms which are constantly floating about us unseen, and which, in their mode of propagation, and in the rapidity with which they multiply and die out, simulate the conditions by which the spread of contagious diseases appears to be influenced.

This view has lately been supported by Dr. Frankland in his Memoir on Sewerage, published in the "Quarterly Journal of Science" for July, and it has this, at least, to recommend it, that the same preventive measures which would be advantageous for diminishing the growth of Fungi, seem also the ones most efficacious in arresting the course of Epidemics.

The third Essay gives a modified support to the Darwinian theory, by pointing out, that in the Vegetable Kingdom reproduction by the union of the sexes seems to have for one of its aims the bringing about of variations from the primitive type, and might thus be to a certain extent capable of accounting for the existence in nature of allied forms, which we are in the habit of recognising as distinct species, and which, according to the rules agreed upon amongst naturalists, we are bound to regard as such, until they can be shewn to produce a fertile progeny.

The Essays I have brought out on subjects relating to Rural Economy would follow next in order, but I have made room for only one of them, namely, that "On the Deterioration of the Soil by continued Tillage," which seemed to me worth preserving, as it exposes the fallacy of the "Humus theory," which, in spite of Liebig's opposition, still seems to find favour in certain quarters.

I have next been induced, by a natural vanity, to insert

the two Addresses which it devolved upon me to deliver at Cheltenham in 1855, and at Tiverton in 1862, as records of the distinctions I obtained in being made President of the British Association at the former place, and of the Devonshire Association for the Advancement of Science, Literature, and Art, at the latter.

The former presents a sketch of the more important advances made of late years in Chemistry, Botany, and Geology, the three Sciences with which I am most familiar; the latter is generally confined to the topics most likely to interest a Devonshire audience, as being illustrated by phenomena presented in their own county or neighbourhood. But, in alluding in p. 84 to a spring which issues from a mine in Cornwall, it points out what appears to me an important distinction between those mineral waters which owe their high temperature, as in the case referred to, to the internal heat of the Globe, and those which appear to derive it from those chemical processes in which volcanic action may be supposed to originate.

The presence of the full complement of oxygen in the former, and its absence or deficiency in the latter, seem to me to shew, that the chemical re-actions going on in various parts of the interior of the Globe are essentially connected with oxidation; whilst the mere internal heat of the Globe, whatever may be its origin, is at present something entirely distinct and independent in its nature, producing no change in the composition of the Gases, which water percolating to these great depths carries down along with it.

The Address also contains some Remarks upon the doctrines of Darwin, which may be regarded as complementary to those contained in the Essay on the Sexuality of Plants, and in my Address to the British Association at Cheltenham.

It may, perhaps, be thought that on this last occasion I have stated too broadly as a general law of Nature, that a tendency exists in varieties to revert to the primeval type; but if apparent exceptions to this can be pointed out in the

persistency of certain races, it must be recollected on the other hand, that as the Darwinian is allowed to draw so largely upon time for the purpose of carrying out his theory of the transmutation of species, his opponents may also fall back upon the same principle, in order to reconcile these exceptional cases, with the more general recurrence to the original stock in the produce of a seed obtained from an improved variety, and with other facts of the same description.

But, at any rate, these are questions which belong exclusively to the domain of Science, and hence I have entered my protest, in the Address given to the Devonshire Association, against the interference of theologians on a subject in which they have really no concern.

Of other productions of my pen, such as my Inaugural Lectures on Chemistry and Botany, only the titles are given, as the information contained in them has either been superseded by subsequent advances in knowledge, or will be found conveyed in other parts of the volume.

The concluding portion of the volume contains a miscellaneous collection of Essays upon subjects connected with Theology, general Literature, and questions relating to University Education.

The first in the series, entitled Christianity and Rationalism in their Relations to Natural Science, was first published as a separate pamphlet for reasons stated in the accompanying Preface.

As the opinions current in the University naturally take their colour from those which are prevalent in the outer world, it seemed desirable to enter a protest against the tendency of the age to transfer the groundwork of our faith from the solid foundations of reason and argument, to the shifting quicksands of imagination and feeling, which is the more likely to be adopted by young minds, because it does not seem *in limine* to involve a disbelief in an unseen world.

It is perhaps natural, that an age like the present, so occupied in investigating the laws of nature, should be apt to ignore those occasional deviations from them which a higher philosophy might recognise, and that the *idola theatri* which men devoted to Science, whether moral or physical, are too apt to cherish, should interfere almost unconsciously with the reception of truths which lie beyond the ken of their ordinary speculations.

Still it must be regarded as strange, that at a time when the colour of a vestment, the propriety of employing incense, or some shadowy distinction of doctrine, is exciting so much interest and discussion, the question of Miracles should be so generally passed over, as if it were comparatively unimportant.

It reminds me of the state of feeling existing in the Byzantine Empire, where the feuds between the green and blue factions in the circus engaged the grave attention of the Court and Citizens of Constantinople, whilst the Persian was thundering at their gates, and the very existence of civilization seemed to be in jeopardy [d].

To me it appears, that the establishment of the reality of Miracles is the one thing needful for the maintenance of Christianity, and that all questions, as to the extent of Inspiration, the reception of particular Dogmas, and the limits of Church authority, sink almost into insignificance by its side.

If their reality, or even their validity, as evidences of the truth of Revelation, be once denied, that of Prophecy will soon follow; for what is the latter, but a miraculous power of foreseeing future events, imparted with the same object, and under similar limitations.

Hence Christianity will, at best, be nothing more than one out of many modes in which the Almighty may have been pleased to impart to the human race a dim insight into His Being and attributes, through the medium of those in-

[d] Gibbon, vol. vi.

stincts and aspirations which are common to us all, and which, indeed, are shared by the untutored Indian, and by the bigoted Mussulman, in a still larger measure perhaps than by the civilized European.

If this be the prevailing sentiment, Christianity is indeed drifting into the position occupied by the religions of the ancient world in the age of Cicero or Tacitus, when, as Gibbon tersely expressed it, all modes of belief were regarded as equally true by the vulgar, equally false by the Philosopher, and equally useful by the Statesman; nor can we find much fault with the Poet, when he says :—

> "E'en gods must yield, religions take their turn,
> 'Twas Jove's, 'tis Mahomet's, and other creeds
> Shall rise in other times, till man shall learn,
> Vainly his altar rears, his victim bleeds,
> Poor child of doubt and death, whose hope is built on reeds."

With these sentiments, it was natural that I should hail with satisfaction the able argument of Mr. Mozley in defence of Miracles; and although, since the publication of my remarks, a vigorous onslaught has been made upon his Work by no less a person than Dr. Tyndall, I am not disposed to retract anything that I had then said in its favour.

In his own department, in which he has achieved so much, I should receive the Professor's *dicta* with the deference due to so distinguished a Philosopher; but on the question of Miracles, I must frankly say, that he does not appear to me to have advanced much beyond the old argument of Hume's, which would lead us to reject any fact not in accordance with general experience.

As, however, our own experience only extends to what the Deity may think fit to ordain with reference to the common course of human events, I see nothing which can justify us in setting aside the testimony which may be adduced in support of the fact, that at the time of the

introduction of a new religion, extraordinary means were resorted to in order to meet an extraordinary emergency.

Dr. Tyndall, indeed, maintains, that as other Religions have gained proselytes without the aid of Miracles, there was no necessity that the order of nature should have been disturbed for the purpose of establishing this; but he overlooks two conditions which the Almighty seems to have imposed upon Himself in the case before us, namely, first, that the new religion should neither be introduced by fraud nor by force[e]; and, secondly, that it should be at all times amenable to the test of reason[f].

I do not, therefore, see how the Christian Religion could have made its way, without the employment of means which the Bible repudiates, or on what principle its rejection, either on its first introduction, or at any subsequent period of its progress, should be so severely denounced, if the aid of Miracles had been denied it.

Let us suppose a Socrates under some mental hallucination, from which the founders of religious sects are not always exempt, to have proclaimed himself sent from God to propagate a new form of faith, and a purer code of morals.

Would the excellence of the doctrines inculcated have been in itself a sufficient reason for accepting his divine mission, or for imputing blame to those who required other proofs of inspiration before admitting it?

Or would a philosopher, who had lived in the early days of Mahometanism, have seen sufficient reason in the triumph of brute force, for admitting the divine origin of a religion which was then extending itself over so large a portion of the globe.

It is true, that as some kind of belief in the unseen is

[e] "I have not written unto you because ye know not the truth, but because ye know it, and that no lie is of the truth."

"My kingdom is not of this world, else would My servants fight for Me."

[f] "Search the Scriptures, prove all things, hold fast that which is good."

natural to man, it would be going too far to maintain that every false religion existing upon the earth has been propagated by violent means, for fraud on the one hand, and credulity on the other, might in many cases explain its success; but as the Creeds of the ancient world, like the races of plants and animals that once lived upon the earth, have, each in its turn, run their course, it may not be rash to assume, that the doctrines of Buddha, of Brahma, and of Mahomet, will hereafter share the fate which has awaited the mythologies of ancient Greece and Rome.

If so, their present existence affords no argument in favour of their divine origin, since if the latter be assumed in any one case, its permanence to the end of time, and its ultimate extension over all parts of the globe, would seem to be involved as necessary conditions; so that it would be requisite that some evidences should exist, which might be appealed to at any future period, when the current of opinion, or the influence of authority, no longer set in favour of a particular Creed.

For the question before us is not the possibility of a new Religion obtaining a footing without the aid of force or of miracle, but of its continuing to command assent, without an appeal to supernatural agency, when the circumstances that favoured its introduction had passed away.

These arguments, I admit, are old enough, but so also are the objections which provoked them, the chief novelty being, that they are urged, not, as formerly, in a scoffing spirit, or by an avowed opponent to Christianity.

And now with regard to the other point dwelt upon in my Essay, I mean the *liberty of prophesying*, as Jeremy Taylor would express it, in religious matters. In discussing this delicate point, I should have done well to allude to an able Sermon of Dr. Hawkins, the Provost of Oriel[g], published many years ago, in which not merely the *right* but even the *duty* of private judgment is maintained; from

[g] On the Duty of Private Judgment.

which it would seem to follow, that, provided suitable pains are taken to arrive at truth, and adequate opportunities have been afforded for investigating it, honest error must, under certain circumstances, be deemed more excusable than uninquiring orthodoxy.

Had I consulted the writings of the same eminent Divine, I should also have found much to confirm and illustrate my position [h], that the divisions in Christendom have in a great degree arisen from the glosses of theologians, who, not content with the plain declarations of Scripture, have too often exalted their own interpretations and developments of them to the same level as the words of Holy Writ.

It might not be difficult to shew, that those pretensions to preternatural powers which the Romish Church puts forth in behalf of her priesthood, and which, I fear, are not always confined to those within her pale, are attributable to the same mistaken procedure; but I am fearful of entangling myself in a maze of theological discussion, when my only purpose was to illustrate the leading proposition, that doctrines revealed to us in the Bible would meet with a readier acceptance, if the human developments grounded upon them were less generally insisted on.

The next Essay is so far related to the one just referred to, that it is an attempt to vindicate a brother Professor from the charge of Infidelity, brought against him on the ground of his disputing the commonly-received opinions respecting Miracles. And I am happy to find, that the view I had taken of his opinions is not only confirmed by the testimony of his surviving friends and relatives, but is also backed by the authority of a well-known orthodox Divine, Canon Woodgate, who, in his Pamphlet entitled "Essays and Reviews Considered," 1861, distinctly states it as his belief, "that Professor Powell's Essay had been greatly misunderstood, and its object misrepresented."

The Memoirs that come next are chiefly devoted to ques-

[h] As in his Sermons on Scriptural Types, 1851.

tions of merely Academical interest, such as the opportunities afforded for the cultivation of Physical Science at Oxford, the relation which these studies bear to each other in the University Curriculum, and the measures necessary for enabling this great seat of learning to become a training-school for youths intended for the Medical Profession, as well as for the Church.

I have also availed myself of this opportunity to place upon record a Protest which I drew up some time ago against an attempt made by some over-zealous individuals to entangle their scientific brethren within the meshes of a Declaration, which, however speciously worded, would have had the effect of checking the progress of truth, by inculcating as a duty the suppression of any facts or deductions resulting from inquiries into Nature, which might appear not to harmonize with Scripture statements on subjects beyond the domain and scope of Revelation.

The Harveian Oration which follows, was given in the Latin language, in compliance with the Regulations then in force at the College.

It has this in common with the brief English sketches of two men distinguished in the literary and the scientific world, which succeed, that it is chiefly a panegyric upon those Members of the Medical Body, who had become remarkable, by the services they had rendered to Physical Science, rather than in their own particular Profession.

On the latter subject, indeed, having been long withdrawn from the practice of Medicine, which was suited neither to my tastes nor habits, I had no right to pronounce a judgment; but considering how much the character and success of its followers depends upon a proper scientific training, I deemed it not inappropriate to single out for notice on that occasion such of the Fellows of the College as had risen to eminence, either as Chemists, as Physicists, or as Natural Historians.

The character I have drawn of the late venerable Presi-

dent of Magdalen College, Oxford, Dr. Routh, was, perhaps, considered by some of his friends, at the time it was originally circulated, hardly sufficiently flattering, but the Public have, I believe, since settled down into the persuasion, that it conveys a pretty fair estimate of his merits and defects.

And if such be the case, it will prove in the long run more advantageous to the memory of a remarkable man, than a mere *éloge*, which, bearing upon its face the undisguised intention of slurring over those weaknesses and shortcomings with which the best of us are chargeable, would command no confidence, and be set aside as the production of a blind admirer, or an undistinguishing partisan.

The succeeding Memoir, on the Celtic Antiquities of Brittany, is an extract from a Lecture on an Archæological subject, a department on which I have much to learn; but I have ventured nevertheless to introduce it, as explanatory of the Remarks contained in my Devonshire Address respecting the antiquity of man.

These Relics are probably the most remarkable, in point of interest and variety, that exist, and deserve more attention than they hitherto appear to have received.

The concluding Essay is of a lighter description than the rest.

A Sexagenarian, who had experienced and lamented the utter want of recognition of Physical Science which prevailed at Oxford in his younger days, evinced amongst other ways in the failure of an attempt made by a small knot of individuals to raise a few thousand pounds by private subscription for the purpose of establishing a Museum, may be excused for expressing a feeling of exultation at the erection, at the University's expense, of a Building, which meets all the reasonable requirements of men of science[1], and im-

[1] As a warm supporter of the contemplated increase to the appliances of our Museum for the accommodation of the Professor of Experimental Philosophy,

plies, that the due position of the latter in the scale of human knowledge is at length, to a certain extent, appreciated in this ancient seat of learning.

The Dream of a new Museum may be regarded in times to come as no very exaggerated picture of the state of feeling that might grow up in our Universities, if that one-sidedness, which, until lately, led us to regard all the elements of a liberal education concentrated in the Classics, were hereafter to be transferred to an equally exclusive admiration of the achievements of Physical Science.

I ought to explain, that if the original plan of the Museum had been adhered to, this addition would not have been required; but that the removal of the Radcliffe Library, which was subsequently determined upon, whilst it relieved the University funds from the charge of providing a Reading-room for the Bodleian, materially curtailed the space which had been originally calculated upon for meeting the wants of the several Professors in the new Building.

CONTENTS.

VOL. I.

PART I.—EXPERIMENTAL ESSAYS.

PART II.—GEOLOGICAL MEMOIRS.

PART I.

EXPERIMENTAL RESEARCHES.

On the Action of Light upon Plants, and of Plants upon the Atmosphere.

(From the Philosophical Transactions for 1836.)

THE researches of Priestley, Ingenhousz, Senebier, Ellis, and above all of the younger Saussure, have long put us in possession of the leading facts appertaining to the influence of light upon the green parts of plants; and Professor Decandolle has embodied the substance of all that had been ascertained on this subject, up to the year 1831, in his admirable work on Vegetable Physiology. But there appear, by the confession of this latter naturalist, to remain certain subordinate questions respecting this same function, which, though perhaps occasionally touched upon by the above-cited experimentalists and by others, can scarcely be said to have as yet obtained a satisfactory reply.

The first of these questions relates to the *nature* of the influence which, in the cases alluded to, is assignable to light. As this agent often produces chemical changes by its direct action upon inorganic bodies, decomposing saline solutions, discolouring oils, and reducing metallic oxides, so it may be supposed to operate directly upon the air, and to possess the power of decomposing carbonic acid, when this substance is presented to it within the pores of the vegetable tissue. And, on the other hand, as light appears to be a specific stimulus to the vital functions of animals, so it may be imagined to act in a similar manner on those of plants, thus enabling them to secrete from the carbonic acid presented to them the carbon required for their nutrition.

Another point as yet undecided relates to the *extent* of the influence it exerts over the vegetable kingdom; or, in other words, the degree in which certain processes attributed to its presence are capable of counteracting others that are going on at all times, whether light be absent or not. Thus, although it may be conceded, as a fact already well established, that plants purify the air in the sunshine, it still remained to be proved, by more decided experiments than had hitherto been instituted, whether the quantity of oxygen given out by them during the day exceeded that absorbed during the night; and moreover, supposing this latter question answered in the affirmative, whether the probable excess was likely to be such, as would afford a counterpoise to the effects produced by animal respiration, combustion, and the like [a].

After considering, therefore, the *modē* in which light appears to affect the functions of plants, I shall naturally proceed to examine the *extent* of the changes produced by the latter upon the air through its influence.

Part I.—*On the Influence of Light upon Plants.*

If of the two modes of considering the operation of light above noticed we adopt the second, that is, if light be supposed to affect plants by a specific stimulus, such as it exerts on animals, and not in the first instance the air as a chemi-

[a] I am aware it may be urged, that the quantity of carbonic acid added, and of oxygen subtracted by these latter means within any moderate period of time, is in itself so small compared with the entire bulk of the atmosphere, that we must not argue, because the constitution of the latter has appeared to continue uniform ever since accurate methods have been devised for determining it, therefore that no change can be taking place insensibly in the proportions of its ingredients.

Still, however, when we recollect how many ages have elapsed since the present races of animals were created, and how many more since the existence of others, which, although extinct, appear from the analogies of their structure to have carried on the same respiratory process which those now in existence fulfil, and therefore could not have endured an atmosphere much more highly charged with oxygen than the present one, we cannot help feeling, that nature must have some means at her disposal, by which the purity of the atmosphere is restored, and its constitution thus maintained without alteration.

cal agent, it ought to follow, that those portions of the spectrum which possess the strongest illuminating power, should exercise upon them the most powerful influence, and produce the most decided effects.

Senebier, however, has stated, that the green colour of leaves, which is supposed to be connected with the decomposition of carbonic acid and the evolution of oxygen, is produced most rapidly under the action of the violet ray[b]: and as the latter, from the feeble light and heat it communicates, seems almost inert, with reference to the functions *of animals,* such a circumstance, if substantiated, would seem strongly to favour the contrary hypothesis.

This latter view of the mode in which carbonic acid is decomposed within the vessels of the plant, would likewise be somewhat confirmed, if it should appear, that whilst the above process was most favoured by violet light, other functions, which are affected by the presence of this agent, but which evidently depend upon a process taking place in the vessels of the plant, are influenced in proportion to the luminousness of the ray; whilst, if the same law were found to prevail in both these cases, and if all the processes alluded to could be proved to go on most rapidly under the influence of the darkest and most refrangible portion of the solar spectrum, a curious difference between the *mode* of its operation upon the vegetable and animal kingdoms might then be suggested.

I felt, therefore, desirous of putting to the test of experiment the two following questions: first, whether the several solar rays act upon plants with equal or with different degrees of energy; and secondly, whether all the functions of plants that seem dependent on the presence of light are affected in the same ratio by similar rays.

Now the following functions are found to depend in great measure on the influence of light.

1. The decomposition of carbonic acid, and the consequent evolution of oxygen, already spoken of.

2. The green colour of leaves, and other analogous parts.

[b] *Mém. de Phys. Chim.*, tom. ii. p. 55.

3. The expansion or unfolding of the leaves in certain species, the folding of which, on the close of day, constitutes what has been called "the sleep of plants."

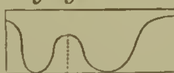
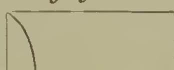
4. The irritability belonging to certain other plants, such as the *Mimosa pudica.*

5. The exhalation of water from the leaves.

6. The absorption of the same by the roots.

The difficulty, however, of comparing the relative intensity of the light transmitted by the various coloured media which were employed in my experiments, induced me to content myself with shewing, that the effect of light upon plants corresponds with its illuminating, rather than with its chemical, or its calorific influence; and to waive the more difficult inquiry, whether its operation upon the vegetable kingdom exactly keeps pace with the increase in its own intensity. And in order to shew that the former is the case, I will in the first place set down the order of sequence of the several media, with reference one to the other, in respect to their illuminating, their calorific, and their chemical power; stating at the same time, by means of a diagram, the rays intercepted and transmitted by each, as was determined for me by Professor Powell; and afterwards proceed to a statement of their respective influence upon plants.

With regard to the means adopted for estimating these points, I need perhaps only remark, that the relative illuminating power of the several media was ascertained by the number of thicknesses of wire gauze, which produced a certain definite degree of obscurity or indistinctness when interposed; their relative calorific power, by the number of degrees which a thermometer with a blackened bulb was raised in a given time by the light transmitted through each; their relative chemical influence, by the time required to reduce paper moistened with a solution of nitrate of silver to a certain standard point of discoloration. The figures in the annexed Table, therefore, must be understood to express nothing more than the *order of sequence,* and not to indicate the ratio between the several media in any of the above respects.

Nature of the media.	Its type.	Its illuminating influence.	Its calorific influence.	Its chemical influence.
Transparent glass	. . . . . .	7	7	7
Orange, No. 5	 *r y g b*	6	6	4
Red, No. 4	 *r y g*	4	5	0
Blue, No. 3	 *r y g*	4	3	6
Purple, No. 2	 *r yg b v*	3	4	6
Green, No. 1	 *r o y g b i v*	5	2	3
Bottle containing a solution of ammonio-sulphate of copper, No. 6 . . .	 *r o y g b i v*	2	1	5
Bottle containing port wine, No. 7	 *r*	1	3	0

Now it remained to be seen, with which of the above scales the power of occasioning a decomposition of carbonic acid in the vessels of the plant, and of forwarding those other functions of vegetable life which depend upon light, most nearly corresponded.

For this purpose, a certain number of fresh leaves, which presented in each case an extent of surface as nearly as possible equal, and had been previously ascertained to give out equal quantities of oxygen, were introduced severally into jars, filled with water impregnated with carbonic acid gas, placed on the surface of a pneumatic trough, and exposed for a certain time to the influence of the solar rays.

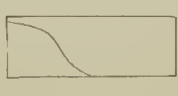
The jars, in which the leaves thus selected stood, were severally covered over by a wooden screen, which intercepted all light from the included jar, excepting in front, where a frame was fitted to it of a nature calculated to sup-

port, either a circular pane of glass, or a flat bottle of corresponding dimensions.

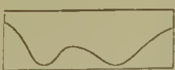
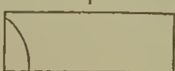
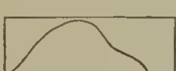
By fixing into the frame the various coloured media with which it was intended to operate, I was enabled to ascertain the influence which the light transmitted through each might exert upon the plant included.

From a variety of experiments, which it seems unnecessary to detail, inasmuch as they merely tend to confirm the statements of preceding observers, it appeared, that the kind of leaf selected made but little difference in the result. I therefore contented myself with selecting such as could be procured most readily, and in the freshest condition.

The first plant operated on was the common Cabbage (*Brassica oleracea*); and as some of the results obtained in this instance appeared anomalous, inasmuch as they indicated an evolution of pure nitrogen, which I have detected in none of my other experiments, three or more trials with each kind of glass were in this instance had recourse to. The following were the results obtained.

Media through which the light was transmitted.	State of the weather.	Proportion between the whole quantity of gas obtained in the two jars.	Proportion per cent. between the oxygen and the nitrogen in the two jars.	Proportion between the oxygen in the two jars.	Proportion between the nitrogen in the two jars.
Jar 1. Transparent window-glass.	*Exp.* 1. Bright sunshine.	Jar 1. 100 Jar 2. 80	O. 44 N. 56 O. 40 N. 60	100 77·5	100 86
Jar 2. Glass No. 5. (Orange.)	*Exp.* 2. Bright sunshine.	Jar 1. 100 Jar 2. 82	O. 33 N. 66 O. 33·4 N. 66·6	100 80	100 83
Type  r y g	*Exp.* 3. Bright sunshine.	Jar 1. 100 Jar 2. 83	O. 0 N. 100 O. 0 N. 83		
Jar 1. Same as before. Jar 2. Glass No. 4. (Crimson-coloured.)	*Exp.* 1. Bright sunshine with a few fleecy clouds.	Jar 1. 100 Jar 2. 45	O. 37·5 N. 62·5 O. 33·3 N. 66·6	100 40	100 48
Type  r y g	*Exp.* 2. Strong bright sunshine.	Jar 1. 100 Jar 2. 54	O. 39 N. 61 O. 38 N. 62	100 52·5	100 55

N.B. The leaves in this latter case were quite fresh, and had not been previously immersed in water.

Media through which the light was transmitted.	State of the weather.	Proportion between the whole quantity of gas obtained in the two jars.	Proportion per cent. between the oxygen and nitrogen in the two jars.	Proportion between the oxygen in the two jars.	Proportion between the nitrogen in the two jars.
Jar 1. Same as before. Jar 2. Glass No. 3. (Dark blue.)	*Exp.* 1. Sunshine with partial clouds.	Jar 1. 100 Jar 2. 57·5	O. 33 N. 66 O. 34·6 N. 65·4	100 56	100 56·5
Type  *r y g*	*Exp.* 2. Bright sunshine without clouds.	Jar 1. 100 Jar 2. 57·5	O. 33·3 N. 66·6 O. 30·2 N. 69·8	100 70·5	100 53·5
Jar 1. Same as before. Jar 2. Glass No. 2. (Purple.)	*Exp.* 1. Sunshine without clouds.	Jar 1. 100 Jar 2. 47	O. 38·4 N. 61·6 O. 30 N. 70	100 35·4	100 51
Type  *r y g i v*	*Exp.* 2. Sunshine more obscured than in Exp. 1.	Jar 1. 100 Jar 2. 31·4	O. 25 N. 75 O. 22 N. 78	100 27·7	100 26·6
Jar 1. Same as before. Jar 2. Bottle No. 7. Filled with port wine. Type  *r y g b i v*	*Exp.* 1&2. Sunshine of different degrees of brightness.	Jar 1. 100 Jar 2. No Gas.			
Jar 1. Same as before. Jar 2. Bottle No. 6. Filled with the ammonio-sulphate of copper.	*Exp.* 1. Sunshine of feeble intensity, though without dense clouds.	Jar 1. 100 Jar 2. 18	O. 47 N. 53 O. 37·8 N. 62·2	100 14·5	100 20·6
Type  *r y g b i v*	*Exp.* 2. Bright & cloudless sunshine.	Jar 1. 100 Jar 2. 49	O. 32·7 N. 67.3 O. 37 N. 63	100 55	100 46
Jar 1. Same as before. Jar 2. Glass No. 1. (Green.)	*Exp.* 1. Bright sunshine with occasional clouds.	Jar 1. 100 Jar 2. 32·8	O. 44 N. 56 O. 34·7 N. 65·3	100 26	100 38
Type  *r y g b i v*	*Exp.* 2. Sunshine, for the most part bright.	Jar 1. 100 Jar 2. 25	O. 34 N. 66 O. 26·7 N. 73·3	100 18	100 25·6
	Exp. 3. Sunshine feeble and intermitting	Jar 1. 100 Jar 2. No gas collected.			

The *second series* of experiments undertaken was with the leaves of the *Salicornia herbacea*, and the following were the results obtained. Temperature 65° Fahr.

Media through which the light was transmitted.	State of the weather.	Proportion between the whole quantity of gas obtained in the two jars.	Proportion per cent. between the oxygen and nitrogen in the two jars.	Proportion between the oxygen in the two jars.	Proportion between the nitrogen in the two jars.
Jar 1. Transparent glass Jar 2. No. 5. Glass (Orange)	Feeble sunshine.	Jar 1. 100 Jar 2. 67	O. 25 N. 75 O. 16·4 N. 83·6	100 44	100 75
Jar 1. Transparent . . Jar 2. No. 4. (Red) .	Feeble sunshine.	Jar 1. 100 Jar 2. 60	O. 10 N. 90 O. 0 N. 60	100 0	100 66·5
Jar 1. Transparent . . Jar 2. No. 3. (Blue) .	Feeble sunshine.	Jar 1. 100 Jar 2. 20	O. 25 N. 75 O. 0 N. 20	100 0	100 26·7
Jar 1. Transparent . . Jar 2. No. 2. (Purple) .	Bright sunshine, with a few clouds.	Jar 1. 100 Jar 2. 16	O. 46 N. 64 O. 15 N. 85	100 5·25	100 21·4
Jar 1. Transparent . . Jar 2. No. 7. Bottle filled with port wine .	Bright sunshine.	Jar 1. 100 Jar 2. 0			
Jar 1. Transparent . . Jar 2. No. 6. Bottle (with ammonio-sulphate of copper) . .	Bright sun, though with a few clouds.	Jar 1. 100 Jar 2. 42	O. 33 N. 66 O. 0 N. 42	100 0	100 63
Jar 1. Transparent . . Jar 2. No. 1. Glass (Green)	Bright sunshine.	Jar 1. 100 Jar 2. 43	O. 46 N. 64 O. 0 N. 43	100 0	100 67

The *third series* undertaken was with common Sea Wrack (*Fucus digitatus*), immersed in water of temperature 65°, and the following were the results obtained.

Media through which the light was transmitted.	State of the weather.	Proportion between the whole quantity of gas obtained in the two jars.	Proportion per cent. between the oxygen and nitrogen in the two jars.	Proportion between the oxygen in the two jars.	Proportion between the nitrogen in the two jars.
Jar 1. Transparent glass Jar 2. No. 5. (Orange)	*Exp.* 1. Feeble sunshine.	Jar 1. 100 Jar 2. 31	O. 55·5 N. 44·5 O. 14·2 N. 85·8	100 7·9	100 59
	Exp. 2. Bright sunshine.	Jar 1. 100 Jar 2. 19 2	O. 60 N. 40 O. 44 N. 56	100 14	100 27

Media through which the light was transmitted.	State of the weather.	Proportion between the whole quantity of gas obtained in the two jars.	Proportion per cent. between the oxygen and nitrogen in the two jars.	Proportion between the oxygen in the two jars.	Proportion between the nitrogen in the two jars.
Jar 1. Transparent . . Jar 2. No. 4. (Red) .	Bright sunshine.	Jar 1. 100 Jar 2. 10·8	O. 75 N. 25 O. 46 N. 54	100 6·65	100 23
Jar 1. Transparent . . Jar 2. No. 3. Glass (Blue)	*Exp.* 1. Bright sunshine.	Jar 1. 100 Jar 2. 13	O. 69 N. 31 O. 31·5 N. 68·5	100 6	100 28·7
	Dull day.	Jar 1. 100 Jar 2. 7·3	O. not determined. O. 0 N. 7·3		
Jar 1. Transparent . . Jar 2. No. 2. Glass (Purple)	Feeble sunshine.	Jar 1. 100 Jar 2. 12·5	O. 60 N. 40 O. 8·8 N. 91·2	100 1·8	100 28·5
Jar 1. Transparent . . Jar 2. No. 6. Bottle (containing a solution of ammonio-sulphate of copper)	Bright sunshine.	Jar 1. 100 Jar 2. 8	O. 70 N. 30 O. 19 N. 81	100 2	100 21·7
Jar 1. Transparent . . Jar 2. No. 1. (Green) .	*Exp.* 1. Weak sunshine.	Jar 1. 100 Jar 2. 7·6	O. 40 N. 60	100	100 7·6
	Exp. 2. Strong sunshine.	Jar 1. 100 Jar 2. 5·2	O. 65 N. 35	100	100 5·2

Bottle No. 7 with port wine tried without effect.

The *fourth series,* with leaves of *Tussilago hybrida,* in water of temp. 70°, gave the following results.

Media through which the light was transmitted.	State of the weather.	Proportion between the whole quantity of gas obtained in the two jars.	Proportion per cent. between the oxygen and nitrogen in the two jars.	Proportion between the oxygen in the two jars.	Proportion between the nitrogen in the two jars.
Jar 1. Transparent glass Jar 2. Glass No. 5. (Orange)	Sultry day.	100 56	O. 45 N. 55 O. 41 N. 59	100 51	100 60
Jar 1. Transparent . . Jar 2. Glass No. 4. (Red)	Ditto.	100 49	O. 44·5 N. 55·5 O. 61 N. 59	100 67·5	100 39
Jar 1. Transparent . . Jar 2. Glass No. 3. (Blue)	Ditto.	100 41	O. 62 N. 38 O. 33 N. 66	100 22	100 72

Media through which the light was transmitted.	State of the weather.	Proportion between the whole quantity of gas obtained in the two jars.	Proportion per cent. between the oxygen and nitrogen in the two jars.	Proportion between the oxygen in the two jars.	Proportion between the nitrogen in the two jars.
Jar 1. Transparent . . Jar 2. Glass No. 2. (Purple)	Sultry day.	100 10·7	O. 54 N. 46		
Jar 1. Transparent . . Jar 2. No. 1. (Green) .	Ditto.	100 14	O. 53 N. 47 O. none, or barely any.		
Jar 1. Transparent . . Jar 2. Bottle No. 6, containing the copper solution	Ditto.	100 21	O. 50 N. 50 O. 13 N. 87	100 5·5	100 20·5

Fifth Series, with leaves of *Cochlearea Armoracia* immersed in water having a temperature of 72°, gave the following results.

Media through which the light was transmitted.	State of the weather.	Proportion between the whole quantity of gas obtained in the two jars.	Proportion per cent. between the oxygen and nitrogen in the two jars.	Proportion between the oxygen in the two jars.	Proportion between the nitrogen in the two jars.
Jar 1. Transparent glass Jar 2. No. 5. (Orange)	Bright sunshine. Th. 80°.	100 75	O. 57 N. 43 O. 54 N. 46	100 71	100 80
Jar 1. Transparent . . Jar 2. No. 2. (Blue) .	Ditto.	100 24	O. 57 N. 43 O. 27 N. 73	100 11·4	100 40
Jar 1. Transparent . . Jar 2. No. 5. (Green) .	Ditto.	100 14·5	O. 57 N. 43 O. 20 N. 80	100 5·25	100 26·8
Jar 1. Transparent . . Jar 2. No. 4. (Red) .	Ditto.	100 15	Not measured. O. 31 N. 69		
Jar 1. Transparent . . Jar 2. No. 3. (Purple) . Jar 3. No. 1. (Green) . Jar 4. No. 6. (Bottle with copper solution)	Ditto.	100 36 20 12	O. 52 N. 48 O. 35 N. 65 O. 16 N. 84 O. 5 N. 95	100·0 24·2 6·2 1·2	100 48 35·2 22·5

N.B. In another experiment with No. 6 no gas at all was collected.

Sixth series, with sprigs of *Mentha viridis* immersed in water of the temperature of 74° Fahr.

Media through which the light was transmitted.	State of the weather.	Proportion between the whole quantity of gas obtained in the two jars.	Proportion per cent. between the oxygen and nitrogen in the two jars.		Proportion between the oxygen in the two jars.	Proportion between the nitrogen in the two jars.
Jar 1. Transparent glass	Cloudless sky. Thermometer 80°.	100	O. 59	N. 41	100	100
Jar 2. No. 5. (Orange)		70	O. 37	N. 63	44	108
Jar 3. No. 2. (Blue) .		22·5	O. 8	N. 92	30	50·5
Jar 4. No. 6. (Bottle with copper solution)		20·0	O. 4	N. 96	13·5	47·0
Jar 1. Transparent glass	Ditto.	100	O. 47	N. 53	100	100
Jar 2. No. 1. (Green) .		20	O. 7	N. 93	3	34
Jar 3. No. 3. (Purple) .		20	O. 0	N. 100	0	40
Jar 4. No. 4. (Red) .		30	O. 12	N. 88	7·7	49

Similar experiments made upon leaves of *Rheum Rhaponticum,* of *Allium ursinum*, and of various speeics of Meadow-grass, corroborated the same conclusions; as likewise did some on plants confined in atmospheric air, containing about six per cent. of carbonic acid, and exposed to these several media.

In the above experiments, the proportion of oxygen was ascertained by heating the air in a bent graduated tube with phosphorus, and observing the diminution of capacity thereby occasioned, two per cent. being allowed for the expansion caused in nitrogen by phosphorous vapour. This method, which I have always found to give very uniform results, I adopted in preference to that of exploding the gas with hydrogen, as being less troublesome and more expeditious, in a climate so damp as ours, than a process requiring the aid of electricity.

The constant presence of more or less nitrogen in the air emitted by the plant, is a circumstance which, although often before observed, deserves, nevertheless, here to be briefly adverted to.

Its quantity appeared to be relatively smaller in proportion to the intensity of the solar influence, being always least under transparent glass; and where the light transmitted was not energetic enough to cause any emission of oxygen at all, still some portion of nitrogen would frequently be given out. Perhaps this circumstance may admit of

explanation, by considering the emission of gas from leaves, when exposed to light under water, as derived from two sources; the first the disengagement of a portion of atmospheric air which it had previously absorbed, and whose place within the tissue of the plant is probably supplied, either by the water with which it is surrounded, or by the carbonic acid with which this water is impregnated; the latter, the emission of pure oxygen, derived from a decomposition of the carbonic acid in contact with it.

Hence in Experiment 1 with cabbage-leaves, where we obtained 100 parts of a gas consisting of oxygen 44, and nitrogen 56 parts, we may suppose that the leaves had emitted,

Of atmospheric air which had been previously absorbed .	68 parts : consisting of	nitrogen	56
		oxygen	12
together with excess of oxygen	32	Excess of oxygen	32
Total of gas obtained	100	Total of oxygen	44

whereas, when orange-coloured glass No. 5 was employed, we obtained,

Of atmospheric air . . .	60·5	Oxygen	12·6
Excess of oxygen	19·4	Excess of oxygen .	19·4
Total of gas . .	79·9	Total of oxygen . .	32·0

The two most difficult cases to explain seem to be, first, the evolution of pure nitrogen, and secondly, that of the same gas accompanied with a smaller proportion of oxygen than that present in the atmosphere.

In the instance in which the former was observed, no incipient putrefaction could be suspected by way of accounting for its occurrence, for the plants were fresh and healthy; and the circumstance that gas is not disengaged at all in the dark, proves the evolution of nitrogen to be in both cases a process connected with the same kind of action as that to which the emission of oxygen is to be ascribed.

Perhaps the phenomenon may be better understood by reference to the experiments of the younger Saussure, which go to prove; that oxygen becomes fixed in the plant in a condition, such as renders it incapable of being withdrawn from the vegetable tissue by the air-pump, or by other mechanical means; that it there unites with the carbon, so

as to bring the latter into a fit state for the plant to assimilate it; and that it is then again disengaged from its combination, by a process not unaptly compared by the late Professor Burnett to the digestion of animals.

Now for this latter function to be discharged, the stimulus of the more luminous rays may be requisite, whilst that of the duller portions of the spectrum máy suffice for the mere respiration of the plant, or for the elimination of the residuary air. Hence, when rays of the latter description are alone transmitted, the composition of the gas evolved may even indicate a smaller amount of oxygen than that present in atmospheric air, because a portion of this element had become combined with certain of the carbonaceous principles present in the vegetable tissue, or been fixed in some manner within the plant.

The other processes, enumerated as under the influence of solar light, appear to be subjected to the same law, as that by which we have seen the decomposition of carbonic acid in the green parts of plants to be regulated.

From a few experiments I have made on the secretion of green matter in the leaves, I should be led to infer, in contradiction to the results of Senebier, that the most luminous rays were most influential; the orange glass, whose chemical influence was as 4, whilst its illuminating power was as 6, quickly imparting to the primordial leaves of beans which had just appeared above ground a bright green hue, whereas under the ammonio-sulphate, whose illuminating power was as 2, whilst its chemical influence was as 5, they continued of a pale yellow, scarcely indeed of a shade darker than in another case where light was completely excluded.

I have made some experiments with similar results on the colours of flowers, the intensity or depth of which appeared also to depend on the brightness of the kind of light that had been allowed admission to them.

The irritability of the Sensitive Plant was likewise found to be dependent on the influence of bright rays, and not of those which act chemically. Six healthy sensitive plants were introduced in the beginning of August into an oblong

box, with partitions so arranged, that each pot was in contact with a differently-coloured light. In five weeks' time, that which had been exposed to the full light of the sun, transmitted through transparent glass, was still excitable, as was the one covered with the orange-coloured ; but those which had received merely the portions of light transmitted through the copper solution, through port wine, and through a solution of green muriate of copper, as well as one which had been kept in entire darkness, lost all their irritability. Yet in each case the temperature was kept up to the same point by means of a hot-bed.

The exhalation of moisture from the leaves, and the absorption of it by the roots, are the last processes dependent on the action of light to which my attention has been directed. The results of my experiments on these two points confirm the same general inference as that to which the foregoing ones point; but having met with some apparent anomalies, I shall forbear at present to report the numerical results of the respective trials made with various glasses. It will be sufficient to lay before the Society a statement of the plan on which the experiments were conducted, and to particularize one or two which tend to shew, that the processes above alluded to are probably dependent on the combined action of heat and light, coupled with those mechanical influences, which operate upon dead, as well as upon living organic matter.

The method whereby I proposed to estimate the degree, in which the exhalation of moisture depended on the quality of the light admitted to the plant, was in itself sufficiently simple. It consisted, in placing some plant growing in a pot, in a square tin vessel, the margin of which received in a groove a cucumber frame of sufficiently large dimensions to inclose it. All communication with the external air was cut off, by means of a little oil introduced into the groove into which the edges of the frame dipped, and the moisture exhaled was absorbed by concentrated sulphuric acid, placed in shallow earthen pots, along with the plant, in the interior of the tin vessel.

By weighing these vessels, just before the plant was introduced, and immediately after it had been taken out, I hoped to ascertain the amount of water that had been evolved, and after deducting from the sum total the quantity which had been previously found to be given off by the plant in the dark, I concluded that the remainder ought to represent the quantity to be set down to the action of light. But as it was impossible to command an uniform intensity of solar radiation during the whole period occupied by any one series of experiments, another plant of the same kind and size was placed under transparent glass; and from the comparative amount of moisture emitted by it, I calculated what might be the difference in the amount of solar influence during the period at which the experiments were carried on.

Now although the experiments conducted on the above plan in general tended to shew, that the extrication of moisture, *cæteris paribus,* was most abundant in proportion to the intensity of light admitted, (orange glass in general causing more moisture to be exhaled than red or green,) yet in some instances blue and purple glasses, and still more remarkably, bottles filled with the cupreous solution, would cause a more abundant exhalation than orange or even transparent glass. Here, however, another principle seems to come into play, namely, the influence of heat radiated from the surface of the screen. This I infer, first, because the quantity of water exhaled under the influence of the copper solution became greatest, when the state of the weather was such as to elevate the temperature of the liquid considerably above that of the surrounding atmosphere; and, secondly, because a bottle filled with water blackened with ink to such a degree, as to transmit just as much light, so far as could be measured by the eye, as that filled with the copper solution was found to do, caused an equally considerable amount of water to be evolved by the plant.

Thus I selected two plants of the Tree Mallow (*Lavatera arborea*), which, by a previous experiment, had been found to exhale in the open air equal quantities of moisture, and placed the one under a frame, into which were inserted the

bottles of ink and water, and the other under one with the solution of ammonio-sulphate of copper. Both fluids soon acquired in the sun a temperature from 110° to 120° Fahr.; and at the end of two hours the sulphuric acid in connexion with each of the plants was successively weighed, and the increase found to be nearly uniform; that under the ink and water having gained 150 grains per hour, that under the cupreous solution 162 grains in the same time.

Now as water with the addition of a little ink is known to absorb the rays proceeding from all parts of the spectrum in an equal ratio, it follows, that the effect produced in either instance must be ascribed to the heat radiated, and not to any peculiar virtue of the violet extremity in stimulating the vegetative functions.

Yet it is curious that the presence of some light seems essential to the due continuance of this process. The same plants which had been employed in the preceding experiment were placed out in the sun on a bright day; one, as before, under the influence of the light transmitted through the cupreous solution, the other under a frame covered over with blue tiles, which, together with the liquid, soon became heated by the sun's rays to the temperature of 110° to 120° of Fahrenheit. At the end of a certain time the sulphuric acid contained in the tin vessel which had inclosed the plant exposed to the action of the violet end of the spectrum had gained at the rate of 159 grains in the hour, (which is within 3 grains of the amount obtained in the previous experiment,) whilst that in connexion with the one covered with the tiles had only increased by 32 grains.

Thus it would appear, that although heat assists the process, some degree of light is essential to its activity.

I was desirous, likewise, of ascertaining whether the brightest kind of light attainable by artificial means contributed in any degree to the process under consideration; Professor Decandolle having found that the leaves of plants placed in a cellar became green on exposure to a strong light from lamps, and that their flowers even reversed their natural periods of opening, when the cellar was illuminated by night, and kept dark during the day.

In my own experiments, the light employed was that produced by a jet of mixed oxygen and hydrogen directed upon a ball of quick-lime, a kind of light, which I have found capable, like that from the sun, of passing through, and being concentrated by, a lens. Nevertheless, in two or three experiments, each lasting nearly an hour, in which the rays proceeding from the incandescent lime were directed towards, and thrown back upon the plant, by concave metallic reflectors, no increase in the quantity of moisture exhaled could be detected, beyond what the same individual had given out whilst in the dark.

The last function which it was left for me to consider, namely the absorption of water by the roots, is so related to the preceding one, that it might almost be inferred *à priori* to be subject to the same laws.

It seems indeed evident, that, *cæteris paribus*, in proportion to the velocity with which the sap ascends, will the extremities of the roots absorb moisture from the ground; since, unless the former operation continued, the latter organs would very soon become fully charged with humidity, and thus the absorption be put a stop to.

In order to ascertain the quantity of water absorbed under different circumstances by the roots of plants, I made the following experiments.

Two small plants of *Helianthus annuus*, in pots marked A and B, were immersed in tin vessels nearly full of water, the height of which within was measured by glass tubes cemented into them below, and rising on the outside nearly to the top. These vessels were severally provided with tin covers, each of which had a circular aperture at its centre, through which the stem of the plant passed, and another small one at the side, through which water might be introduced. Being elsewhere closely attached to the vessel, little or no evaporation could take place from the surface of the included water.

Things being thus prepared, the two plants were placed for twenty-four hours in the open air, part of the time exposed to a bright sun, with the thermometer at 75° in the shade.

At the expiration of that time it was found that the vessels had lost, as nearly as eould be aseertained, the same quantity of water, whieh amounted in eaeh case to four ounees.

The next day, the thermometer being as on the preeeding oeeasion, and the sun bright, sunflower A was plaeed under a frame glazed with orange-eoloured glass the same as No. 5, and sunflower B under one glazed with blue glass as No 2.

In the evening it was found, that the tin vessel eontaining plant A had lost three ounees of water, and that eontaining B one ounee. Now in another experiment with the same plants, the tin eontaining A, plaeed under a frame glazed with blue glass No. 2, had lost six ounces; that eontaining B, under one glazed with orange, No. 5, had lost 9½. So that although the ratio between the two was very different (being in the one ease as 1 to 3, in the seeond as 1 to 1½), still in both there was a manifest superiority in the orange over the blue glass with respeet to its power of produeing absorption. Now orange-eoloured glass seemed to aet with about half the energy whieh belonged to transparent; for in another experiment upon the same plants, whilst the *former* had eaused an absorption of four ounees, the *latter* had oecasioned one of only two.

The same plants being next tried, one with a eovering of transparent, the other with one of red glass, it was found that the former had absorbed 4½, the latter 2 ounees, in equal times, the ratio being as 2·00 to ·89.

Henee the following may be stated as representing the relative amount of absorption in these several eases:

Under transparent	2·00
Under orange	1·00
Under blue, varying from . . .	·33 – ·66
Under red	·89

Similar experiments were tried with Vines, and with the *Sagittaria sagittifolia*, whieh latter being an aquatie plant, eontinued for a longer period in a healthy eondition when immersed in the water. But in either instanee the same exception was found with respeet to the influenee of light on the rate of absorption, as had been observed in that of ex-

halation, those glasses which radiated most heat, appearing to act upon the plant with an energy quite disproportionate to their illuminating power.

The heat of the weather was very great during these experiments, the thermometer being frequently above 80°, and the liquor in the bottles often mounting as high as 105°, so that it would seem, that the heat radiated from the coloured liquid assisted in promoting absorption in the roots, as it appeared to have done in the former one in increasing the exhalation from the leaves. The same might have been the case, though in a lesser degree, when the blue or red glasses were the media employed, and thus certain irregularities observed in the results may perhaps be explained, by supposing that the joint action, of the light transmitted, and of the heat radiated from these screens, caused a greater exhalation from the leaves, and thereby produced a more abundant absorption by the roots to supply the deficiency.

Upon the whole, then, I am inclined to infer, from the general tenor of the experiments I have hitherto made, that both the exhalation and the absorption of moisture by plants, so far as they depend upon the influence of light, are affected in the greatest degree by the most luminous rays, and that all the functions of the vegetable economy, which are owing to the presence of this agent, follow in that respect the same law. And this is just what we ought to expect, if we suppose that light acts upon the vegetable, as it does upon the animal kingdom, in the character of a specific stimulus; for we are all sensible, that the vivifying influence of light upon ourselves is in proportion to its brightness; nor is it uninteresting to remark, that rays of the very description which most abound in solar light, are at once the most cheering to the animal creation, and the most conducive to the growth and well-being of the vegetable.

We have already seen, that even that most intense form of artificial light which is emitted from incandescent lime, produces no sensible influence upon plants; and we are reminded, that the same holds good with respect to animals, by the fact said to have been observed by the exhibitors of the oxyhydrogen microscope, namely, that animalcules of

kinds which used to be speedily destroyed by the too stimulating action of solar light, appear to suffer much less from that now substituted, provided the water in which they are immersed does not become heated thereby.

Part II.—*On the Action of Plants upon the Atmosphere.*

Having now considered the mode in which light may be supposed to operate upon plants, I shall next proceed to examine the extent of the changes wrought in the constitution of the atmosphere by the latter, under the influence of this agent.

I say the extent of the changes produced, because no one denies the nature of this operation, or questions the fact, that carbonic acid is really decomposed, and oxygen given out, by the green parts of plants under certain circumstances. But between the original views of Priestley, who saw in this process a counterpoise to the effects of animal respiration and the like, and those of Ellis, who did not admit it even as an equivalent to the opposite tendency of vegetable respiration, as carried on during the absence of solar light, a wide difference exists, and hence some fresh investigations seem necessary with reference to this question.

On perusing the account, which Mr. Ellis, in the second part of his "Researches on Air," has given of the experiments by which he attempts to establish this latter opinion, several circumstances occurred to me, of a nature calculated to throw doubts upon their conclusiveness. I may mention, for example, in the first place, the smallness of the volume of air in which his plants were confined; in the second, the length of time that was suffered to elapse before the air underwent examination, owing to which it is even stated occasionally that the leaves had begun to fade and drop off; in the third place, the removal in some cases, and the neglect of a due supply in all, of that carbonic acid, the decomposition of which would have constituted the very source of the oxygen which it was expected to discover.

Accordingly I kept constantly in view these three essential conditions, and contrived an apparatus, in which the quantity of air should be so large, that the healthy functions of the plant might be as little as possible interfered with; in which the constitution of the air could be examined as frequently as I pleased; and in which a regular supply of carbonic acid could be kept up, without disturbing the plant, or suspending the progress of the experiment by its introduction.

The apparatus consisted of a large bell-glass jar, containing in one case 600, in the other 800 cubic inches of air[c], and suspended by pulleys. Its edges dipped into quicksilver, contained in a double iron cylinder of corresponding dimensions to the jar, which being closed at bottom, constituted a well of about six inches in depth, calculated to receive a fluid, and to admit of the glass vessel moving freely in it. The inner margin of this hollow cylinder was cemented air-tight, according to circumstances, either to a plate or a pot of iron, upon which the plant operated on might be placed; and the jar was then let down upon it, until its edges were sunk a little beneath the surface of the mercury.

Thus all communication with the external atmosphere was cut off, and the effect of the plant upon the air inclosed in the jar was readily measured, by simply pressing down the latter, and thus expelling a portion of its contents through a tube, communicating with its interior, and introduced at its outer extremity under a pneumatic trough, wherein the air might be collected, and examined. By connecting this extremity with a vessel containing a measured quantity of carbonic acid, and raising the jar a little in the well of mercury, it was easy to draw in any proportion of that gas, with which it was thought proper that the plant should be supplied. A portion of the air was always tested, immediately after the introduction of every fresh portion of carbonic acid, and again after an interval of some hours, and the proportion of this gas and of oxygen present was carefully

[c] Larger jars, containing from 1,200 to 1,300 cubic inches, were latterly employed.

registered. The amount of carbonic acid was determined by a solution of potash, that of oxygen by the rapid combustion of phosphorus with a portion of it in a bent tube.

Such was the mode of procedure, when an entire plant became the subject of experiment; but some of the most satisfactory trials were with branches of certain shrubs, themselves too large to be admitted under the jar. These branches, without being detached from the parent trunk, were introduced through a hole in the centre of two corresponding semicircular plates of iron, which were cemented air-tight, to the inner margin of the iron cylinder on the one hand, and to the stem of the branch on the other. In this manner, when the jar came to be placed over them, and to dip beneath the surface of the mercury, the external air was as effectually excluded, as it had been when the whole of the plant was inclosed.

The results of several experiments conducted after this plan will be given in a tabular form; but it may be well in the first instance to specify one of the most satisfactory of those undertaken. In this case the jar itself contained about 600 cubic inches of air, and the plant experimented on was the common Lilac (*Syringa vulgaris*). The proportion of carbonic acid in the jar was each morning made equivalent to 5 or 6 per cent. by additions through the tube.

The first day no great alteration in the air was detected, but on the second day, by eight in the evening, the oxygen had risen to 26·5 per cent. In the morning it had sunk to 26, but by 2 P.M. it had again risen to no less than 29·75, and by sunset it had reached 30 per cent. At night it sunk one-half per cent.; but the effect during the following day was not estimated, as the sickly appearance which the plant now began to assume induced me to suspend the experiment.

In a second trial, however, the branch of a healthy Lilac growing in the garden was introduced into the same jar, where it was suffered to remain until its leaves became entirely withered.

The first day the increase of oxygen in the jar was no

more than 0·25 per cent., but on the second it rose to 25·0. At night it sunk to nearly 22 per cent., but the next evening it had again risen to 27 per cent. This was the maximum of its increase, for at night it sunk to 26, and in the morning exhibited signs of incipient decay. Accordingly in the evening the oxygen amounted only to 26·5; the next evening to 25·5; the following one to 24·75; and the one next succeeding it had sunk to the point at which it stood at the commencement, or to 21 per cent.

The reason of this decrease was, however, very manifest in the decay and falling off of the leaves; so that this circumstance does not invalidate the conclusion which the preceding experiments concur in establishing, namely, that in fine weather, at least so long as the plant continues healthy, it adds considerably to the oxygen of the air when carbonic acid is freely supplied.

In the last instance quoted, the exposed surface of all the leaves inclosed in the jar, which were about fifty in number, was calculated at no more than three hundred square inches, and yet there must have been added to the air of the jar as much as twenty-six cubic inches of oxygen, in consequence of the action of this quantity upon the carbonic acid introduced.

But there is reason to believe, that even under the circumstances above stated (which were more favourable to the due performance of the functions of the plant than those to which Mr. Ellis's were subjected), the amount of oxygen evolved was much smaller than it would have been in the open air, for by introducing several plants into the same jar of air in pretty quick succession, I have succeeded in raising the amount of oxygen contained from 21 to 39 per cent., and probably had not even then attained the limit to which the increase of this constituent might have been brought.

How great, then, must be the effect of an entire tree in the open air under favourable circumstances! and we must recollect that, *cæteris paribus*, the circumstances will be favourable to the exertion of the vital energies of the plant, within certain limits at least, in proportion as animal re-

spiration and animal putrefaction furnish to it a supply of carbonic acid.

Neither is this influence exerted exclusively by plants of any particular kind or description. I have found it alike in the monocotyledonous and the dicotyledonous, in such as thrive in sunshine and those which prefer the shade, in aquatic as well as in terrestrial, in cryptogamous and imperfect, such as Ferns and Algæ, as well as in those of a more complicated organization. How low in the scale of vegetable life this power extends is not yet exactly ascertained, but Professor Marcet of Geneva, in a late paper, has shewn that it does not prevail amongst Fungi.

The disappearance of carbonic acid in my experiments always more than kept pace with the addition to the quantity of oxygen; but the shortness of time during which the plant could be retained in a sufficiently healthy condition, prevented my ascertaining, whether after the carbonic acid had been absorbed by it, a part was not at some subsequent period given out again unchanged.

A small portion might perhaps have been taken up by a thin film of water, which I was compelled to keep continually upon the surface of the mercury, in order to prevent the latter from destroying, by a disengagement of its vapour, the plant confined underneath it. This quantity, however, must have been inconsiderable compared with the amount introduced.

I shall now conclude, by placing in a tabular form some of the principal, or the more illustrative experiments which I have carried on, appending some remarks immediately suggested by the particular phenomena observed in each.

Experiments concerning the influence of plants on atmospheric air mixed with various proportions of carbonic acid; the plants being exposed to the sun, and confined in jars containing from 600 to 800 cubic inches of air, and which rested upon mercury covered by a thin film of water.

EXPERIMENT 1.

Date.	Circumstances of the experiment.	Proportion per cent. of carbonic acid added.	Absorption per cent. caused by solution of potash.	Residuum per cent. after burning phosphorus in a portion of the air.	Allowance for phosphorous vapour.	Calculated amount per cent. of nitrogen.	Calculated amount per cent. of oxygen.
April 10.	12 A.M. A small Cypress in a garden pot was introduced into the jar, its stem being cemented air-tight into the hole in the two hemispherical iron plates, that fit the inner margin of the hollow cylinder	0	0	81	2	79	21
	6 P.M. After a cloudy and gloomy day, with only occasional gleams of sunshine	0	0	81	2	79	21
April 11.	8 A.M.	0	0	79	2	77	23
	8½	8	8				
	5. P.M. Stormy and cloudy day, much like that preceding it	0	0	79·5	2	77·5	22·5
April 12.	12 A.M. No sun during the morning, but a settled rain throughout . . .	0	0	81·5	2	79·5	20·5
	1 P.M.	4	4				
April 13.	8 A.M.	0	0	82	2	80	20
	12 A.M. A fine bright day, with occasional storms .	3	3				
	4 P.M.	0	1	79	2	77	23
April 14.	The unfavourable state of the weather induced me to suspend the experiment.						

Remarks.—The circumstances most worthy of remark in this experiment appear to be, 1st. the emission of 2 per cent. of oxygen the second day, when the proportion of carbonic acid in the air of the jar was too small to be detected by my apparatus; and 2ndly, the absorption of carbonic acid afterwards when no oxygen was evolved. In the first case we must suppose that the plant had imbibed from the atmosphere, previously to its confinement, the carbonic acid, which under the influence of sunshine it decomposed within the jar; in the second, that it absorbed a large quantity of carbonic acid, which in the unfavourable state of the weather it did not decompose. This latter supposition must

be adopted as applicable to most of the succeeding experiments; for my apparatus was proved to be sufficiently perfect to prevent any such escape of carbonic acid, within a corresponding period of time, introduced into the jar, when no plant was present in it, and thereby to obviate any suspicion that it might arise from a defect in the union of the joints.

Experiment 2.

Date.	Circumstances of the experiment.	Proportion per cent. of carbonic acid added.	Absorption per cent. caused by solution of potash.	Residuum per cent after burning phosphorus in a portion of the air.	Allowance for phosphorous vapour.	Calculated amount per cent. of nitrogen.	Calculated amount per cont. of oxygen.
April 19.	2 P.M. A dull day, with a tendency to rain. Stem and branches of a Persian Lilac (*Syringa persica*) introduced into the jar in the manner above described	9	9	81	2	79	21
	7 P.M.	0	2·75	78	2	76	24
April 20.	8 A.M. Air examined before the sun had acquired any power	0	2·75	80	2	78	22
	11 A.M.	3·25	6·00				
	7 P.M. After a dull day, with occasional storms until 2 P.M., when the sun broke out	0	4·25	80	2	79	21
April 21.	Observed the leaves to be altered and faded, and this, together with the unfavourable state of the weather, induced me to discontinue the experiment.						

Observations.—It appears from this experiment, that when a plant is in a perfectly healthy and fresh condition, it may add considerably to the amount of oxygen even in dull weather.

Experiment 3.

April 26.	12 A.M. Dull day, with occasional gleams of sunshine, but no rain. A (*Pelargonium*) Geranium with its pot introduced into the jar	5	5	81	2	79	21·0
	7 P.M.	0	1	79·5	2	77·5	22·5
April 27.	8 A.M.	0	0	79·25	2	77·25	22·75
	The unfavourable state of the weather led me to discontinue this experiment.						

EXPERIMENT 4.

Date.	Circumstances of the experiment.	Proportion per cent. of carbonic acid added.	Absorption per cent. caused by solution of potash.	Residuum per cent. after burning phosphorus in a portion of the air.	Allowance for phosphorous vapour.	Calculated amount per cent. of nitrogen.	Calculated amount per cent. of oxygen.
April 29.	12 A.M. Dull and stormy day, with occasional gleams of sunshine. A Geranium with its pot introduced into the jar, occupying together a capacity of about 60 cubic inches	5	5	81	2	79	21
	6 P.M.	0	1	83·6	2	81·6	18·4
April 30.	10 A.M.	5	5	82·5	2	80·5	19·5
	5 P.M. After a bright day, with clouds and showers occurring occasionally	0	0	78·5	2	76·5	23·5
May 1.	8 A.M. Access of the morning sun prevented by a screen covering the jar .	0	2				
	A diminution in the quantity of oxygen was detected, but its amount was not registered, as the apparatus was found not to be perfectly air-tight. The experiment was therefore suspended in order to repair the defect.						

EXPERIMENT 5.

May 8.	12 A.M. Bright day. A Geranium in a pot was introduced into the jar .	5	5	81	2	79	21·00
	1½ P.M.	0	2·5	79·75	2	77·75	22·25
	2½	0	2·25	79.25	2	77·25	22·75
	3½	0	0·05	79·00	2	77·00	23·00
May 9.	8 A.M. During the morning screened from the sun	0	0·0				
	10 A.M. Fine bright day .	10	10·0				
	11 A.M.	0	9·50	82	2	80·00	20·00
	12 A.M.	0	6·25	82	2	80·00	20·00
	2 P.M.	0	6·5	80	2	78·00	22·00

EXPERIMENT 6.

May 10.	Fine bright day. At 12 A.M. a fresh Geranium was introduced	10·5	10·5	81	2	79	21
	5½ P.M.	0	1·0	76	2	74	26

Experiment 7.

Date.	Circumstances of the experiment.	Proportion per cent. of carbonic acid added.	Absorption per cent. caused by solution of potash.	Residuum per cent. after burning phosphorus in a portion of the air.	Allowance for phosphorous vapour.	Calculated amount per cent. of nitrogen.	Calculated amount per cent. of oxygen.
May 11.	11 A.M. Dull and sultry day. Fresh Geranium introduced	8·5	8·5	81	2	79	21
	11 P.M.	0	1	81	2	79	21

Observations.—These two latter experiments shew decidedly that the disappearance of carbonic acid is not at all in proportion to the disengagement of oxygen. In Experiment 6, in a bright sun, the Geranium, which absorbed 9·5 per cent. of carbonic acid, emitted 5 per cent. of oxygen; in Experiment 7, a similar plant, which absorbed 7·5 of the former in a dull day, seemed to have made no addition at all to the amount of oxygen in the jar.

Experiment 8.

May 13.	2 P.M. Bright sunshine all the day. A young Lilac (*Syringa vulgaris*) had its stem introduced under the jar, its roots being in a pot outside	8	8	81	2	79	21
	$7\frac{1}{2}$ P.M.	0	0·05	82·5	2	80·5	19
May 14.	8 A.M.	0	6·00	not esti	mated.		
	$8\frac{1}{2}$. Day as that preceding it	2	8				
	8 P.M.	0	2	75.5	2	73·5	26·5
May 15.	9 A.M. Jar during the preceding part of the morning covered with a screen	0	3·5	76·0	2	74	26·0
	$9\frac{1}{2}$ A.M.	6·5	10·0				
	2 P.M.	0	3·0	72·25	2	70·25	29·75
	$8\frac{1}{2}$ P.M.	0	0·0	72·00	2	70·00	30·00
May 16.	8 A.M. Jar during the former part of the morning having been covered with a screen	0	2·75	72·50	2	70·50	29·50
	The plant beginning to look unhealthy was now removed.						

Observations.—The remarkable clearness of the sky, and the warmth of the weather during these three days, was peculiarly favourable to the experiments, and will account for the large quantity of oxygen obtained, which was equal to 8·5 per cent., or 8·5 × 6 = 51 cubic inches. Owing to the longer time that the plant continued in a state of health, the quantity of carbonic acid that disappeared did not so much exceed that of the oxygen given out as in the preceding experiments.

Experiment 9.

Date.	Circumstances of the experiment.	Proportion per cent. of carbonic acid added.	Absorption per cent. caused by solution of potash.	Residuum per cent. after burning phosphorus in a portion of the air.	Allowance for phosphorous vapour.	Calculated amount per cent. of nitrogen.	Calculated amount per cent. of oxygen.
May 20.	4 P.M. Day dull, but free from rain: in the evening a little sunshine. Branch of a Lilac, having on it about fifty healthy leaves, each leaf on an average presenting a surface of about six square inches (50 × 6 = 300 square inches to the whole)	5	5	81	2	79	21
	7 P.M.	0	3·3	80·75	2	78·75	21·25
May 21.	7½ A.M. Jar during the former part of the morning covered by a screen .	0	2·25	80	2	78	22
	11½ A.M. Bright sunny day. Thermometer 65° .	0	2·25	79·5	2	77·5	22·5
	3½	0	0·00	77	2	75·0	25·0
May 22.	8 A.M. Before uncovering the jar	0	0·5	79.75	2	77·75	22·25
	9 A.M. Fine bright cloudless day. Therm. 68° .	8·5	9·0				
	1½ P.M.	0	6·0	75·75	2	73·75	26·25
	5 P.M.	0	3·0	74·5	2	72·5	27·5
May 23.	8 A.M. Before the screen had been removed . . .	0	2	75	2	73	27
	8 P.M. After a fine bright day	0	0	76	2	74	26
May 24.	8 A.M. Before removing the screen	0	0	75·5	2	73·5	26·5
	4 P.M. After a fine bright day	0	0	76·5	2	74·5	25·5
May 25.	8 A.M. Before removing the screen	0	2	76·75	2	74·75	25·25
	6 P.M. Fine bright day, like yesterday	0	0	77·25	2	75·25	24·75
May 27.	10 A.M.	0	1	80·75	2	78·75	21·25
	Experiment discontinued, as most of the leaves had dropped off, and the remainder looked decayed and withered. The decay of the leaves began to date from the 24th, at which time the amount of oxygen will be seen to have begun to diminish.						

Observations.—This experiment shews how erroneous a conclusion we might deduce, if we contented ourselves with examining the air after an interval of some days, and how soon, even under the most favourable circumstances, confinement interferes with the healthy functions of a plant.

Experiment 10.

Date.	Circumstances of the experiment.	Proportion per cent. of carbonic acid added.	Absorption per cent. caused by solution of potash.	Residuum per cent. after burning phosphorus in a portion of the air.	Allowance for phosphorous vapour.	Calculated amount per cent. of nitrogen.	Calculated amount per cent. of oxygen.
Aug. 20.	12 A.M. Day dull and cloudy, with a few occasional gleams of sunshine. Therm. 70°. A young Cedar with its pot was introduced under a jar having a capacity of about 800 cubic inches .	7.5	7·5	81	2	79	21
	10 P.M.		2.75	80	2	78	22
Aug. 21.	10 A.M. Day overcast, but with occasional gleams of sunshine. Thermometer 70°	0	2·0	82·5	2	80·5	19·5
	10½	7·5	9·5				
	7 P.M.	0	5·0	80	2	78	22·0
Aug. 22.	11 A.M. Windy day, but with a bright sun till about 4 P.M. Thermometer 70°	0	4·0	80	2	78	22.0
	7 P.M.	0	0·0	78·75	2	76·75	23·25
Aug. 23.	8 A.M. Windy and cloudy day, with occasional showers, and few gleams of sunshine. Therm. 64°	0	2·0	80	2	78	22
	10 A.M.	7·5	9·5	80	2	78	22
	7 P.M.	0	4·0	78·50	2	76·50	23·50
Aug. 24.	10 A.M. Cloudy day, without sun. Therm. 66° .	0	1	77·25	2	75·25	24·75
	12 A.M.	7·5					
	7 P.M.	0	5	78·20	2	76·20	23·80
	Experiment discontinued.						

Experiment 11.

Aug. 20.	12 A.M. Bright day with occasional clouds. Therm. 72°. Another Cedar like the former, with its pot, was introduced into a jar containing about 1300 cubic inches of air; size of the plant being 8 inches in diameter, branches and all, and 16 inches from the pot to the topmost branch	7·50	7.50			79	21
	10 P.M.		2·00	80	2	78	22
Aug. 21.	10 A.M. Windy day, often overcast		1·75	79·50	2	77·50	22·50
	10½ A.M.	7·50	9·25				
	7 P.M.		5·00	79·50	2	77·50	22·50

Experiment 11 (*continued*).

Date.	Circumstances of the experiment.	Proportion per cent. of carbonic acid added.	Absorption per cent. caused by solution of potash.	Residuum per cent. after burning phosphorus in a portion of the air.	Allowance for phosphorous vapour.	Calculated amount per cent. of nitrogen.	Calculated amount per cent. oxygen.
Aug. 22.	11 A.M. Windy day, but with a bright sun . . .		2·50	77·0	2	75·00	25·00
	7. P.M.	0	1·0	75·5	2	73·5	26·5
Aug. 23.	8 A.M. Windy and cloudy day, as above stated . .	0	1·0	78·0	2	76	24·0
	10 A.M.	7·5					
	7 P.M.	0	6·0	75·25	2	73·25	26·75
Aug. 24.	10 A.M. Cloudy day, without sun.	0	5·0	77·25	2	75·25	24·75
	12 A.M.	2·5	7·5				
	7 P.M.	0	6·0	75·25	2	73·25	26·75
	Experiment was here discontinued						

Observations.—These two latter experiments tend to shew that the leaves of evergreens purify the air as well as those of deciduous plants, although they may not act upon the carbonic acid with the same energy as the latter do.

Experiment 12.

May 27.	Bright sunny day. Therm. 60°. *Crassula lactea*, with its pot, introduced into the jar at 12 A.M. . . .	·11	11			79	21
	6 P.M.	0	6·5	81·25	2	79·25	20·75
May 28.	8 A.M. Jar previously covered with a screen . .	0	6·0	81·25	2	79·25	20·75
	5 P.M. After a bright day	0	6·5	80·00	2	78·00	20·00

Observations.—This experiment, and one made on another species of Crassula, shew that these succulent plants do not act with much energy on the air, at least under confinement; but the following experiment evinces that even this tribe operates in the same manner, though not with the same intensity.

Experiment 13.

June 30.	12 A.M. Plant of *Mesembryanthemum verrucosum* introduced into jar. (N.B. No record of the weather: probably it was fine) . .	6·6				79	21
	8 P.M.		4·9	78·6	2	76·6	23·4
July 1.	10 A.M.		2·0				
	10½ A.M.	4·5	6·5				
	5 P.M.	0	5·0	82·5	2	80·5	19·5
2.	1 P.M.	0	5·0	79·5	2	77·5	22·5
3.	12 A.M.	0	2·0	80·75	2	78·75	21·25
4.	12 A.M.	3·25	2·25	81·75	2	79·75	20·25
	8 P.M.		5·00	81·5	2	79·5	20·50
5.	1 P.M.		2·00	81·25	2	79·25	20·75
7.	12 A.M.		1·00	83·75	2	81·75	18·25

Observations.—Before the experiment was brought to a close, the plant had

assumed an unhealthy appearance; yet the length of time during which it was confined, and the smallness of the change it produced upon the included air, shew the more languid manner in which the functions of vegetable life are carried on through the medium of the metamorphosed leaves of this tribe of plants. A similar experiment was tried with a specimen of *Aloe mitræformis,* in which case also 2 per cent. was added to the amount of oxygen by its presence.

Experiment 14.

Date.	Circumstances of the experiment.	Proportion per cent. of carbonic acid added.	Absorption per cent. caused by solution of potash.	Residuum per cent. after burning phosphorus in a portion of the air.	Allowance for phosphorous vapour.	Calculated amount per cent. of nitrogen.	Calculated amount per cent. of oxygen.
Aug. 30.	12 A.M. Healthy Dahlia in a pot, containing one full-blown flower and twenty leaves, was introduced into the jar	7·5				79	21
	8 P.M. After a day, bright till 3 P.M., afterwards overcast		3·0	79·25	2	77·25	22·75
Aug. 31.	10 P.M. Bleak and boisterous weather throughout the day, with much rain. Therm. 65°. . .	0	2·0	77·25	2	75·25	24·75
Sept. 1.	Weather cold, stormy, and bleak, but a bright sun occasionally. Thermometer 55° at 9 A.M. . . .	7·5					
	At 10 P.M.		2·0	78·25	2	76·25	23·75
Sept. 2.	Fine day but cloudy: sun at intervals bright. Thermometer 62° at 10 P.M. .	0	1·0	79·25	2	77·25	22·75
Sept. 3.	Dull stormy day at 9 A.M.	7·5					
	At 4 P.M.		4·0	77·25	2	75·25	24·75
Sept. 4.	Fine day at 1 P.M. . . .		0·0	75·50	2	73·50	26·50
	Experiment suspended, though the plant seemed to have suffered but very little from its confinement.						

Experiment 15.

Aug. 30.	Weather reported above. Healthy Dahlia, with about as many leaves as the former one, but without any flower, was introduced into the jar at 12 A.M.	7·5				79	21
	8 P.M.	0	3·75				
Aug. 31.	10 A.M..	0	3·75	82·25	2	80·25	20·75
	10 P.M..	0	2·00	82·75	2	80·75	19·25

EXPERIMENT 15 (*continued*).

Date.	Circumstances of the experiment.	Proportion per cent. of carbonic acid added.	Absorption per cent. caused by solution of potash.	Residuum per cent. after burning phosphorus in a portion of the air.	Allowance for phosphorous vapour.	Calculated amount per cent. of nitrogen.	Calculated amount per cent. of oxygen.
Sept.	10 A.M.	7·5	9·50				
1.	10 P.M.		7·50	78·00	2	76·00	24·00
2.	10 P.M.		5·00	79·00	2	77·00	23·00
3.	4 P.M.		4·00	78·25	2	76·25	23·75
4.	1 P.M.		1·00	78·00	2	76·00	24·00
	Experiment was here suspended, as I was on the point of leaving home.						

Observations.—These two last experiments shew that even plants which are in flower may purify the atmosphere, the influence of the accompanying leaves more than counterbalancing that of the parts of fructification: and, indeed, a comparison of the results of the first experiment with those of the second seems to indicate, that the presence or absence of a flower does not make so great a difference as might have been anticipated.

The same experiment was tried with a plant of *Helianthus annuus* in full flower, and an increase of 1 per cent. of oxygen was detected.

In the following experiments a succession of different plants was introduced, at intervals of from four to twelve hours, into a jar containing about 240 cubic inches of common air, together with generally about 10 cubic inches of carbonic acid. The following Table will shew the increase of oxygen caused.

EXPERIMENT 16.

Date.	Circumstances of the experiment.	Oxygen. Proportion per cent.	Oxygen. Calculated number of cubic inches.
May 23.	Before any plant was introduced	21	50
	After a pot of *Sinapis alba* had continued in contact with the air during six hours of bright sunshine	25	60
	After a pot containing *Hydrangea hortensis*, only in leaf, had continued in contact with the air during twelve hours of bright sunshine	26·5	63·5
May 24.	After a pot containing a *Crassula lactea* had continued in contact with the air during four hours of bright sunshine	32·75	77
	After a pot of *Lepidium sativum* had continued in contact with the air during four hours of bright sunshine	33·5	80·5
May 25.	After a pot containing a Geranium (*Pelargonium*) had continued in contact with the air during five hours' bright sunshine	36·5	87·0

EXPERIMENT 16 (*continued*).

Date.	Circumstances of the experiment.	Oxygen. Proportion per cent.	Oxygen. Calculated number of cubic inches.
May 25.	After the same *Hydrangea* as before had continued in contact with the air during five hours' bright sunshine	33·25	80·0
May 27.	After a Geranium had continued in contact with the air during five hours' bright sunshine . . .	37·0	88·0
	After a healthy Myrtle had continued in contact with the air during four hours' bright sun . . .	39·0	93
May 28.	After a young but apparently healthy plant of the Sweet Pea (*Lathyrus odoratus*) had continued in contact with the air for four hours	33	78·5
May 30.	After a Geranium had continued in contact with the air for five hours	36·5	87·0

Observations.—Here, owing to an accident, the experiment was suspended; but it was carried far enough to shew how much oxygen may be added to the air during the day by plants in a healthy condition. The increase was progressive until I employed a second time the same plant: it probably had suffered from its previous confinement, or from being passed backwards and forwards through the water of the pneumatic trough on which the jar rested. The only other plant which diminished the amount of oxygen was the Sweet Pea; all the others added somewhat to its quantity.

EXPERIMENT 17.

Capacity of jar 620 cubic inches (600 atmospheric air, 20 carbonic acid).

Aug. 7.	Before any plant was introduced	21	126
	6 P.M. After a Geranium had continued in contact with the air during five hours' strong sun . . .	23	138
Aug. 8.	10 A.M. After a second Geranium had remained in contact with the air during the previous night and morning	22	132
	5 P.M. After a Myrtle had been in contact with the air during the period since the previous experiment made	24·5	147
Aug. 9.	11 A.M. After a second Myrtle had continued in contact with the air ever since the last experiment	26	156
	5 P.M. A common garden Lettuce growing in a pot, having continued in contact with the air since the last experiment: day fine	29	174
Aug. 10.	11 A.M. A second Lettuce, having continued in contact with the air since the preceding experiment: day wet	30	180
	5 P.M. After a Geranium had been introduced since the preceding experiment: day wet . . .	35	210
Aug. 11.	11 A.M. After a second Geranium had continued in contact with the air during the interval since the last experiment	28	168
	Finding a temporary difficulty in procuring healthy plants, I here suspended the experiments.		

Observations.—This latter series of experiments was undertaken in order to shew, that even when the plants were confined in the air of the jar, by night, as well as by day, the balance was still greatly in favour of their purifying influence.

With respect to the comparative influence of similar plants in direct and in diffused light, I have as yet made only a few experiments, but the results of these few seem to indicate that even under the latter circumstances an increase in the amount of oxygen will be sometimes produced by them.

	Per cent.
Thus a jar, in which a Geranium had been confined, contained, after exposure for four hours to direct light, of oxygen .	25
Whilst another, in which a similar plant had been placed for the same time in diffused light, contained . . .	19·5
On the other hand,	
Two jars, containing Myrtles, after being placed in direct light for eight hours, had acquired of oxygen,	
No. 1.	23·75
No. 2.	26 00
Two similar jars with Myrtles in diffused light, exposed for the same time, contained,	
No. 1.	20 0
No. 2.	23 0
Two jars with *Polypodium dryopteris*, the one in direct, the other in diffused light, each contained of oxygen . . .	22 0

In conclusion, it may perhaps be well to present in a tabular form the different questions which this inquiry embraces, pointing out how far each may be considered as answered, either by the experiments of preceding investigators, or by those detailed in the present memoir.

SCHEME OF EXPERIMENTS.

Part I.—ON THE ACTION OF LIGHT UPON PLANTS.

1. Solar light. A. direct. 1. In causing the leaves to emit oxygen, and to decompose carbonic acid . .	Immersed in water. Tried with 1. *Brassica oleracea;* 2. *Salicornia herbacea;* 3. *Fucus digitatus;* 4. *Tussilago hybrida;* 5. *Cochlearia Armoracia;* 6. *Mentha viridis;* 7. *Rheum rhaponticum;* 8. *Allium ursinum;* and several species of *Gramineæ.* In atmospheric air. Tried with Geraniums.

2. To become green when etiolated — Tried with Beans.
3. To maintain their irritability . . — Tried with the Sensitive Plant (*Mimosa pudica*).
4. To exhale water by their leaves — Tried with Vines, Dahlias, Helianthuses. *Lavatera arborea*, &c.
5. To absorb the same by their roots — Tried with plants of *Helianthus annuus*, *Sagittaria sagittifolia*, Vines, &c.

B. diffused or reflected. — Its influence compared with that of direct solar light in the above particulars. — Tried with the Geranium, Myrtle, and Polypodium, so far as regards its relative influence in causing the emission of oxygen.

II. Artificial light, obtained A. from lamps. Tried by Professor Decandolle.

B. from incandescent lime. — Tried by myself, but no influence detected.

Part II.—ON THE ACTION OF PLANTS UPON THE ATMOSPHERE.

I. Proportion between the effects attributable to their action during the night and during the day.

1. During fine weather, and in bright sunshine.

		Maximum increase per cent. of oxygen.
Of plants without flowers, and with leaves alone, viz.	Cupressus	.2
	Cedrus	.3·75
	Common Lilac . .	.8·75
	Ditto	.6·50
	Pelargonium . . .	.2·00
	Ditto	.5·00
	Crassula, 2 sp. . .	.0·00
	Mesembryanthemum	2·40
	Dahlia	.3·00
Of plants with flowers and leaves.	Dahlia	.3·75
	Helianthus . . .	.1.00

2. During bad weather, or in diffused light. — Persian Lilac 3 per cent. Geranium, Myrtle, Fern, as noticed above.

II. Proportion between the carbonic acid absorbed and oxygen evolved. — My experiments shew that when plants are confined the former is always greatest at first; but this may not continue to be the case after a certain interval.

III. Greatest amount of oxygen that can be added to the air of a jar by the influence of a plant. — My experiments shew that at least 18 per cent. of oxygen may be so added.

IV. At what stage in the scale of vegetable life the function of purifying air stops. — Probably where there cease to be leaves.—I have shewn that it exists in dicotyledonous and monocotyledonous, in evergreens and deciduous, in terrestrial and aquatic plants, in the green parts of succulents as well as in ordinary leaves, in Algæ and in Ferns as well as in phanerogamous families.
Prof. Marcet has shewn that it does not take place in Fungi.

APPENDIX.

These researches have been confirmed by Dr. Draper, Professor of Chemistry in the University of New York, who, in his treatise "On the Forces which produce the Organization of Plants," published in 1844, states that he had determined the quantity of oxygen given off, when each portion of the sunbeam was directed steadily upon a plant for several hours by means of an Heliostat.

The results arrived at by this more precise method were exactly similar to my own, the most luminous portion of the spectrum causing the greatest evolution of gas, and the chemical as well as the heating rays being by themselves inefficient.

Thus the oxygen set free

In the extreme red ray varied from .	·0	to	0·33
Red and orange . . .	20	,,	24·75
Yellow and green . . .	36	,,	43·75
Green and blue	4·1	,,	0·10
Blue	1·0	,,	0·0
Indigo	·0		
Violet	·0		

Clöetz and Gratiolet also, in 1851, repeated the same experiments on water-plants, and found, that in their influence upon them, the yellow rays stood highest in the order of precedence, then the red, then the green, and lastly the blue.

The experiments detailed by Dr. Gladstone in the Reports of the British Association for 1853, 1854, and 1855, which I shall afterwards allude to, corroborate the same conclusion so far as concerns the growth and development of a plant subsequent to its germination; and lastly Dr. Sachs, Professor at the University of Bonn, in his "Handbook of Experi-

mental Physiology," published at Leipsic in 1865, has communicated a general report of various researches which had been instituted by himself on this subject.

Those relating to the influence of the solar rays on the disengagement of oxygen, entirely confirm the conclusions I had arrived at.

The method in which his experiments were conducted was as follows:—

Carbonic acid was passed into water contained in a cylindrical glass vessel, in which the sprig of a living plant was floating.

This cylinder was enclosed within another, large enough to admit of a space of nearly half an inch between the two, into which, in one instance, a solution of bichromate of potash; in another, one of ammonio-sulphate of copper; and in a third, pure water was introduced.

The effect produced upon the plant by the rays which passed through each of these media was estimated, by counting the number of bubbles of air which were disengaged from it in a minute; and the chemical influence exerted at the same time by the sunlight, was measured by the time required for producing a certain amount of blackness on paper impregnated with a solution of nitrate of silver exposed to its action within a similar pair of cylinders.

Under these circumstances *Ceratophyllum demersum* gave out, under yellow light, 23 bubbles per minute; under white, nearly the same; under violet, none at all: and yet, whilst the yellow light produced no action upon the silver salt, the violet blackened it speedily.

Consistently with this conclusion it was found, that granules of chlorophyll were formed under yellow and white glass, for leaves etiolated in consequence of the absence of the rays transmitted are destitute of this principle. The chlorophyll, however, does not arise from starch present in the cells, for this latter principle, under the influence of light, is found to be converted into gum and sugar. Schubeler also, a Norwegian botanist, states, that plants which he had brought home from a more southern climate, produced larger and deeper coloured seeds, as well as flowers of a darker hue and

more aromatic flavour than before,—circumstances which he attributes to the greater brightness and longer continuance of sunlight during summer in this high northern latitude.

Nevertheless, in spite of all this concurrent evidence, tending to establish that the influence which light exerts upon a living plant is a function wholly or principally of the luminous rays, it is still stated in some standard works, although so far as I can ascertain, without any authority, that the chemical portion of the spectrum is the one most instrumental in promoting the vital processes of vegetation.

On the Growth of Plants confined in Glass Vessels.

(*From the Reports of the British Association for the Advancement of Science for 1838.*)

At the meeting of the British Association for the Advancement of Science for 1836, a Committee was named, consisting of Professor Henslow, Dr. Daubeny, Mr. James Yates, Dr. Henry, and Dr. Dalton, to institute experiments on the growth of plants under glass, and excluded from the external air.

With reference to this investigation, the following letter was communicated by myself to Mr. James Yates, the Secretary of the said Committee.

DURING the last week in April, 1838, I introduced a considerable number of living plants into glass globes, having only a single aperture through which air could circulate, and that one covered over by a sound piece of bladder closely attached to the edges of the glass, so as to preclude the possibility of any air entering the vessel except through the membrane itself.

The following were the plants introduced into these vessels:—

In glass 1 were *Sedum rupestre* and *telephium, Veronica repens, Gentiana acaulis, Erigeron bellidifolium, Lobelia fulgens, Saxifraga virginiana*, and *irrigua.*

In glass 2 were *Primula vulgaris, Anemone nemorosa, Pulmonaria angustifolia, Alchemilla vulgaris, Valeriana dioica, Veronica repens, Lobelia fulgens.*

In glass 3 were *Primula veris* and *auricula, Erigeron bellidifolius, Dianthus armeria, Sempervivum montanum*, and *Lobelia fulgens.*

Now these plants were allowed to remain till May 5th,

a period of almost ten days, undisturbed, at the end of which time they appeared healthy and had grown considerably; some even had flowered since their introduction.

The air contained in each jar was then examined during the day, a portion of it having been drawn off into an exhausted tube through a stop-cock connected with the jar.

In this manner it was ascertained that the air in jar 1 contained 4 per cent. of oxygen more than the proportion present in atmospheric air; in jar 2, 1½ per cent. more; in jar 3, 2 per cent. more.

At night, on the contrary, this excess of oxygen had disappeared, the air examined three hours after sunset corresponding in every case as nearly as possible with that present in the atmosphere.

The following day (May 6th) the results were not equally favourable, yet even then in jar 1 there was an excess of 2 per cent. of oxygen; in jar 2 an excess of 1 per cent.; in jar 3 of 2 per cent., and this excess was plainly attributable to the action of light, for it in a great measure disappeared when the jars were left in the dark for a few hours, No. 1 under this treatment being found to contain just the quantity present in the atmosphere, and No. 2 only 0·75 more.

It would seem, then, that for a certain period plants have the power of thriving and adding to the amount of oxygen, even under the circumstances detailed; but that there is a limit to this power appeared on a re-examination of the air three weeks afterwards (viz. on May 25th), when it was found that jar 1 contained only 1 per cent. more oxygen than that in the atmosphere, instead of 4, as on the 5th instant, and that jars 2 and 3 even contained a portion less.

Examined again on June 20th, No. 1 was found to contain 2½ per cent. less of oxygen than that in atmospheric air; No. 2, 3½ less; No. 3, 4 per cent. less. We seem therefore to have reached the lowest degree of aërial circulation under which plants will continue to live and thrive, although even this slow transmission of air was sufficient for their vitality, rendering it only less vigorous and healthy.

To ascertain, then, what the degree of circulation through the substance of the membrane in these instances might

have been, I removed from one of the jars the plants and vegetable mould it had contained, and substituted for them about an equal amount of dry sand. I then passed through the vessel a current of oxygen until the volume of air within contained no less than 77 per cent. of that gas. The air was then examined again at 4 p.m., after an interval of three hours from the period of the first experiment, and found to have lost 4 per cent. of oxygen. The jar was then put aside till eight o'clock the next morning, when it was found to contain only 63 per cent. of oxygen, having diminished in sixteen hours 10 per cent. After having been exposed all day to air and light, and examined at eight the same evening, the oxygen was found to amount to only 46 per cent., having diminished in 12 hours 18 per cent. During the next night it had diminished in 12 hours only $6\frac{1}{2}$ per cent., the amount of oxygen existing in it the next morning being $38\frac{1}{2}$ per cent. During the next day it had lost 7 per cent., containing at eight in the evening $31\frac{1}{2}$ per cent. The next night the diminution was only $2\frac{1}{2}$ per cent., and on the succeeding day 3 per cent. The following night the diminution was $1\frac{1}{2}$ per cent., the amount of oxygen being $24\frac{1}{2}$ per cent. only. During the day a further diminution of $3\frac{1}{2}$ per cent. took place, the air inclosed within the jar being found to contain exactly the quantity of oxygen present in atmospheric air.

The following is a tabular view of the results :—

June 23rd	1 p.m.	amount of	oxygen	77·	excess	56·
,, ,,	4 p.m.	,,	,,	73·	,,	52·
,, 24th	8 a.m.	,,	,,	63·	,,	42·
,, ,,	8 p.m.	,,	,,	45·	,,	24·
,, 25th	8 a.m.	,,	,,	38·5	,,	17·5
,, ,,	8 p.m.	,,	,,	31·5	,,	10·5
,, 26th	8 a.m.	,,	,,	29·0	,,	8·0
,, ,,	8 p.m.	,,	,,	26·0	,,	5·0
,, 27th	8 a.m.	,,	,,	24·5	,,	3·5
,, ,,	8 p.m.	,,	,,	21·0	,,	0·0

Thus five days were required to enable the whole excess of oxygen to pass through the substance of the membrane, the

diameter of which was 3 inches, whilst the capacity of the vessel, when the sand had been introduced, was nearly one gallon, so that about three quarts of oxygen and one of nitrogen may be calculated as having been present in the jar at the commencement of the experiment, of which about $4\frac{1}{2}$ pints were discharged through the membrane in the course of the five days during which the observations were continued.

The transmission took place more rapidly during the day because of the exposure of the jar to the sun and wind, which by the expansion caused within the vessel, and by the more rapid succession of aërial currents brought into contact with the external surface of the membrane, doubtless caused in a greater degree the transmission of the redundant oxygen. The average quantity that escaped per diem did not much exceed 11 per cent., or did not quite amount to one pint in the 24 hours, but of course the transmission was more rapid at first, and diminished gradually in quantity as the evaporation of the air within the jar approached more nearly to that of the atmosphere surrounding it.

On the Influence of Light upon the Germination of Seeds.

(From the Report of the British Association for the Advancement of Science for 1855.)

AN opinion has gone abroad, and has found a place in several standard treatises[a], that as the luminous rays favour the development of the growing plant, so the chemical rays promote the germination of the seed.

The authority upon which this statement rests, seems to be that of some experiments instituted by Mr. Robert Hunt, who, whilst employed in investigating the chemical action of light upon inorganic bodies, and its application to photography, turned his attention likewise to the influence of the same agent upon plants.

One circumstance alone, however, might raise a doubt as to any direct effect having, in the instances reported, been produced by the several solar rays, namely, that, so far as can be collected from the statement given, all the seeds tried by Mr. Hunt were buried in the ground to the usual depth. Now I fouud that a depth of two inches of common garden soil was quite sufficient to intercept the rays of light, so as to prevent the slightest chemical action being exerted upon highly sensitive paper placed beneath it.

The improbability therefore, of a ray of light acting through such a medium induced me to institute a set of experiments, in which seeds were placed on the surface of moist earth exposed to the action of particular portions only of the solar spectrum.

Although the results obtained are rather of a negative than of a positive description, and have likewise been in some measure superseded by the researches already published by Dr. Gladstone, yet as the experiments were repeated

[a] See in particular Mrs. Somerville's work on Physical Geography.

during the last summer, and led uniformly to similar results, they are communicated, as justifying the conclusion to which I had previously arrived, namely, that no decided influence of a direct kind in promoting germination can be traced to the chemical rays of light, as compared with other portions of the sunbeam.

Six sorts of seeds were in general employed in these experiments, and the number of radicles and plumules of the several kinds which had protruded each day were duly registered.

The media employed for isolating certain rays, or at least particular portions of the spectrum, are enumerated in the table annexed, by reference to which it will be at once seen, what specific luminous influence was exerted upon the seeds by each of those coloured glasses or fluids which are named in the brief statement of the experiments which follow.

I am indebted to Mr. Maskelyne, the Deputy-Reader of Mineralogy at Oxford, for examining the various media employed, and defining by reference to Fraucnhofer's lines the exact quality of the rays transmitted by each, as is stated in the Table.

In the first set of experiments a south aspect was selected, and the following seeds were experimented upon, viz.:—

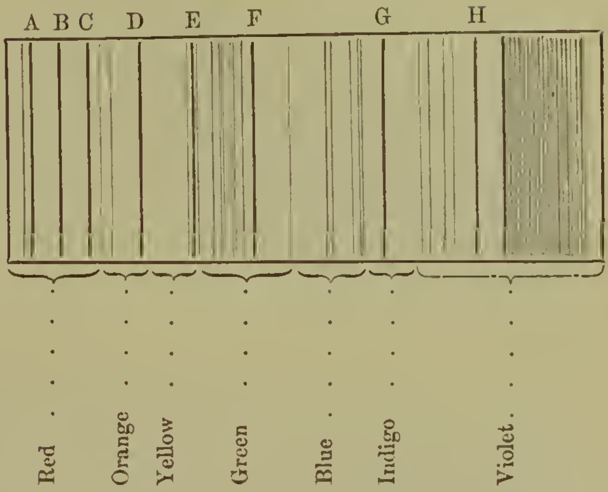
Datura Tatula . .	10	Helianthus annuus .	13
Malope grandiflora .	14	Polygonum fagopyrum	16
Trifolium incarnatum	14	Hordeum sativum .	14
Raphanus rotundus .	12		—
		In all . .	93

But as none of the two first came up, the real number operated upon may be estimated at 69. Of these—

46 radicles and 18 plumules came up under violet light.
44 radicles and 18 plumules came up under green glass.
41 radicles and 19 plumules came up in one instance } in darkness.
41 radicles and 5 plumules came up in another instance } in darkness.
36 radicles and 26 plumules came up under cobalt-blue glass.
32 radicles and 17 plumules came up under amber glass.

FRAUENHOFER'S SPECTRUM.

RAYS ADMITTED BY THE MEDIA.



No. 1. Transparent Glass,
All the rays fully admitted.

No. 2. Blue Glass,
All partially admitted.

No. 3. Dark Green Glass,
Green and a little blue admitted.

No. 4. Light Green Glass,
All deadened.

No. 5. Ruby Glass,
Red and orange admitted.

No. 6. Amber Glass,
All up to green admitted.

No. 7. Orange Glass,
All up to indigo admitted.

Vessels containing

No. 8. Ammonio-sulphate of Copper,
Only the violet admitted.

No. 9. Port Wine,
Only the red admitted.

No. 10. Ink and Water,
All the rays deadened.

No. 11. Black Board,
All the rays excluded.

N.B. The Black lines indicate the rays admitted.

29 radicles and 7 plumules came up under ruby glass.
23 radicles and 5 plumules came up under orange glass.

Accordingly, in this series a slight superiority seemed certainly to belong to the violet-coloured medium over the rest, in relation to the number both of radicles and of plumules which appeared; whilst in respect to the quickness of their germination, the violet and green media were a-head of the rest, although the plumules did not follow the same order.

When, however, the same experiments were repeated in a north aspect, the same law did not hold good, for out of 69 seeds,—

52 radicles and 22 plumules appeared under green glass.
49 radicles and 17 plumules appeared under blue glass.
47 radicles and 14 plumules } appeared in darkness.
47 radicles and 21 plumules }
44 radicles and 17 plumules appeared under transparent glass.
39 radicles and 23 plumules appeared under violet light.

And with respect to the quickness of germination, it appeared that the green stood first in order; that the seeds under blue and violet glass and in absolute darkness came up next in order, and with nearly equal rapidity; that those in full light were next in order; whilst orange, ruby, and yellow were about equal, but somewhat later than the rest.

It did not appear, therefore, from this last series of experiments, that violet light favoured germination at all more than any other species of light; nor indeed that any kind of ray was injurious to the process, so long as its intensity was not too great, as may be inferred to have been the case in the first set of experiments, where the seeds were exposed to the full rays of the sun in a southern aspect.

I therefore, in my subsequent experiments, selected uniformly a north aspect for the germination of the seeds; and in order still further to test the point as to whether the quality of the light had anything to do with the process, I placed as before upon the surface of the soil, in boxes, ten seeds of each of the four following plants, viz. peas, beans,

kidney-beans, and a species of sun-flower (*Helianthus annuus*), all of which germinated. Now in this case

37 radicles and 25 plumules appeared in the dark box;
36 radicles and 30 plumules appeared under green glass;
35 radicles and 30 plumules appeared under blue glass;
34 radicles and 24 plumules appeared under transparent glass;
the whole number of seeds operated upon being only 40.

It would seem, then, as if in these cases the absence or presence of light was almost a matter of indifference.

In the fourth series of experiments rather a greater variety of species was experimented upon, and a larger number of media employed, the total number of seeds in each box being 52, viz. of a species of sunflower, peas, kidney-beans, and barley, 10 of each, and of radishes 12. In this instance, the whole number came up under four of the media employed, but these media were of very different qualities; in one case, all light being excluded; in another, the violet ray alone admitted: in another, green light; and in the fourth, a pale green glass being used, which cut off none of the rays completely, although it enfeebled all.

The number of plumules that were developed in these several instances, were from 46 to 47.

The number of radicles developed under transparent glass was only less by two than the others, so that no fair inference would seem deducible from this series, in favour of one medium being preferable to another. The radicles, however, came up most rapidly in total darkness, and least so when all the rays were admitted.

Although the above four sets of experiments seemed to render it improbable that any influence, favourable or otherwise, could be traced to particular rays or portions of the spectrum, still it seemed desirable to shew more directly, that where the quantity of light was the same its quality was immaterial.

It was with this view principally that I instituted a fifth set of experiments, in which the light was filtered, as it were, through liquids—one of which was the ammonio-sulphate of copper, which excluded all but the violet; an-

other, port wine, which admitted only the extreme red; and a third, a mixture of ink and water, which deadened equally all the rays of the spectrum.

It was in the first place ascertained, as nearly as could be done by the eye, that an equal amount of light was admitted through each of the media, they being severally diluted with water, until they allowed just so much light to pass as was sufficient for reading the largest print in a chamber otherwise darkened.

The results appear to shew, that there was under these circumstances scarcely any difference to be detected; nor indeed did a glass, which admitted all the light present, appear to interfere with the process materially, although in the box from whence light was entirely excluded the germination seemed to go on somewhat less vigorously than in the others.

It will be seen at least, that out of 50 seeds, or 10 of each of the following—radishes, peas, kidney-beans, sun-flower, and barley,—

49 radicles and 48 plumules appeared under port wine.
49 radicles and 43 plumules appeared under ink and water.
47 radicles and 36 plumules appeared under transparent glass.
46 radicles and 48 plumules appeared under ammonio - sul-
45 radicles and 48 plumules appeared under phate of copper.
42 radicles and 37 plumules appeared in total darkness.

Upon the whole, from a general survey of the above experiments, no other conclusion seems deducible, except that light has very little to do directly with the germination of seeds; and that although the popular opinion may be well-founded, namely, that the process goes on best in the dark, as maltsters generally believe, still that the light which interferes with the success of the operation acts chiefly by producing such a degree of dryness as is unfavourable to the sprouting of the seed, and not by itself interfering directly with the result.

An experienced maltster, indeed, assures me, that darkness is not necessary for malting, although, in order to maintain a suitable degree of humidity in the apartment, strong light is generally excluded.

In the Tables annexed, the numbers attached to each column indicate merely the relative number of radicles or plumules, which had been found to develope themselves under the several media employed, on each of the days of which the date is given.

First set of Experiments.—In a South Aspect.—SUMMARY.

Numbers that had vegetated on each day.—Experiment beginning April 13.

Media.	April 17	18	19	20	21	22	23	24	25	26	
No. 1. White	...	...	1	3	5	8	11	15	19	...	P
	...	2	6	10	14	18	22	26	30	...	R
No. 2. Blue	...	...	2	5	8	12	16	21	26	...	P
	2	6	10	14	18	22	26	32	36	...	R
No. 6. Amber	...	...	...	2	4	6	10	14	18	...	P
	1	4	7	10	13	17	22	27	32	...	R
No. 5. Ruby	...	...	...	...	...	...	2	4	7	...	P
	...	2	5	8	11	14	19	24	29	...	R
No. 7. Orange	...	...	...	...	...	1	2	3	5	...	P
	...	2	4	6	8	10	14	18	23	...	R
No. 3. Green	...	...	...	2	4	6	10	14	18	...	P
	4	9	14	19	24	29	34	39	44	...	R
No. 11[a]. Black	...	...	...	2	4	7	11	15	19	...	P
	3	7	11	15	19	24	29	35	41	...	R
No. 11[b]. Black	...	...	...	...	...	...	1	3	5	...	P
	2	6	11	16	21	26	31	36	41	...	R
No. 8. Violet	...	...	...	3	6	9	12	15	18	...	P
	4	9	14	19	24	29	34	40	46	...	R

Second set of Experiments.—In a North Aspect.—SUMMARY.

Numbers that had vegetated on each day.—Experiment beginning April 28.

Media.	May 1	2	3	4	5	6	7	8	9	10	
No. 1. White	...	...	...	1	...	11	...	17	...	...	P
	11	31	38	41	...	43	...	44	...	...	R
No. 2. Blue	...	...	...	...	...	14	...	24	...	...	P
	18	36	43	46	...	48	...	49	...	...	R
No. 7. Orange	...	...	...	...	...	8	...	17	...	...	P
	8	28	33	37	...	37	...	41	...	...	R
No. 5. Ruby	...	...	...	...	...	5	...	14	...	...	P
	8	31	36	38	...	40	...	44	...	...	R
No. 6. Amber	...	...	...	...	...	6	...	8	...	...	P
	8	29	38	41	...	42	...	43	...	...	R
No. 3. Green	...	...	...	...	...	5	...	22	...	...	P
	23	37	49	51	...	52	...	52	...	...	R
No. 11[a]. Black	...	...	...	2	...	3	...	14	...	...	P
	5	28	36	41	...	43	...	47	...	...	R
No. 11[b]. Black	...	...	...	...	...	10	...	21	...	...	P
	15	32	42	45	...	45	...	47	...	...	R
No. 8. Violet	...	...	...	...	...	19	...	23	...	...	P
	16	31	36	37	...	38	...	39	...	...	R

Third set of Experiments.—In a North Aspect.—SUMMARY.

Numbers that had vegetated on each day.—Experiment beginning May 16.

Media.	June													
	3	4	5	6	7	8	9	11	13	15	17	21	23	
No. 1. White . .	...	...	...	...	5	7	7	9	15	21	23	24	24	P
	8	19	19	23	31	33	33	34	34	34	34	34	34	R
No. 2. Blue . . .	...	...	...	...	9	9	10	15	20	24	25	30	30	P
	9	20	20	30	33	35	35	35	35	35	35	35	35	R
No. 3. Dark Green	...	...	...	...	5	10	10	14	20	25	27	30	30	P
	9	20	20	29	33	35	36	36	36	36	36	36	36	R
No. 4. Black . .	...	...	...	...	5	10	12	13	21	23	25	25	25	P
	18	18	18	25	33	33	33	36	36	37	37	37	37	R

Fourth set of Experiments.—In a North Aspect.—SUMMARY.

Numbers that had vegetated on each day.—Experiment beginning July 28.

Media.	August																		
	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	
No. 1. White . . .	...	1	9	16	16	25	25	27	30	35	36	36	37	37	41	41	41	46	P
	20	40	42	42	42	44	44	45	46	46	46	46	46	46	49	49	49	50	R
No. 4. Light Green .	...	...	13	27	27	29	29	30	34	34	37	37	37	37	42	42	42	44	P
	21	38	40	40	40	44	44	45	45	45	45	45	45	45	52	52	52	52	R
No. 3. Dark Green .	...	...	17	20	20	27	27	28	33	35	37	38	40	42	47	47	47	47	P
	23	46	46	47	47	48	48	48	50	50	50	50	50	50	50	50	50	52	R
No. 2. Blue . . .	...	5	16	26	26	27	27	27	32	36	38	38	38	38	46	46	46	46	P
	25	41	43	43	43	43	43	43	43	43	43	43	43	43	48	46	46	16	R
No. 8. Violet . . .	...	...	23	30	30	30	30	40	42	43	43	43	43	43	46	46	46	46	P
	23	42	45	45	45	45	45	45	46	46	46	46	46	46	52	52	52	52	R
No. 11. Black . . .	...	1	10	20	20	22	22	22	27	30	37	37	38	38	47	47	47	47	P
	31	43	45	45	45	47	47	47	48	48	48	48	48	48	52	52	52	52	R

Fifth set of Experiments.—In a North Aspect.—SUMMARY.

Numbers that had vegetated on each day.—Experiment beginning May 25.

Media.	June																	
	2	3	4	5	6	7	8	9	10	11	12	17	19	20	21	23	26	
No. 1. White . . .	...	...	...	3	3	3	10	15	20	26	26	32	32	34	34	35	36	P
	20	26	34	37	37	37	41	41	46	46	46	47	47	47	47	47	47	R
No. 11. Black . .	...	...	3	3	7	13	17	20	20	25	25	29	31	32	32	33	37	P
	...	37	40	40	41	41	41	41	42	42	42	42	42	42	42	42	42	R
No. 8ª. Sulphate of Copper	...	...	...	...	1	2	7	8	11	20	20	28	31	34	34	34	38	P
	...	22	33	33	33	34	36	37	38	44	44	44	44	44	44	44	45	R
No. 9. Port Wine .	...	3	3	7	10	24	27	28	31	35	35	37	37	40	40	42	48	P
	6	36	38	38	40	43	43	43	46	47	47	47	47	47	47	47	49	R
No. 8ᵇ. Sulphate of Copper	...	...	3	3	5	11	18	23	26	33	33	35	36	39	39	40	45	P
	...	32	39	39	39	39	39	41	45	45	15	45	45	45	45	45	46	R
No. 10. Ink . . .	...	...	...	...	...	2	8	12	18	26	26	33	35	38	38	39	13	P
	...	25	29	29	34	38	40	43	48	49	49	49	49	49	49	49	49	R

On Ozone and its Disengagement by the Leaves of Plants.

(From the Journal of the Chemical Society, January, 1867.)

THAT a substance called ozone, characterized by a peculiar smell and by remarkable oxidising properties, may be generated by artificial means, is a fact upon which I presume all chemists are agreed; and as one of the readiest means of obtaining it is by the passage of electrical sparks through air containing oxygen, it can hardly be doubted, that whenever during a thunder-storm the same odour is perceived, it is owing to the generation of this principle by atmospheric electricity.

Ozone, then, must exist in the atmosphere, and this being allowed, we are naturally led to assume its presence, whenever those re-agents which we rely upon for detecting it in an artificial mixture are affected in the same manner by the passing of air over them.

Now, paper soaked in a solution of iodide of potassium with starch is one of the most delicate of these tests: and so, likewise, is the protosulphate of manganese, which has an additional dose of oxygen communicated to it through the same agency, and hence, by the brown colour its solution obtains, indicates the presence of ozone.

Assuming, therefore, in either instance, that the intensity of the resulting colour is an index of the proportion of ozone present in the air, it has become the practice of meteorologists to register its amount by comparing with a scale of colours the tint produced upon the paper by its exposure for a given time to the action of the atmosphere.

It has been objected, indeed, to this conclusion, that other

bodies, which may be supposed to exist in the air, might produce similar reactions. Nitrous acid, for example, which we know to be generated by electrical discharges, would affect either kind of paper, and so likewise would chlorine, which, in the neighbourhood of the sea at least, might be suspected to be present.

I think, however, it may be proved, that neither of these gases existed in the air, which, according to my observations, affected Schönbein's paper in the manner described.

Two tubes were attached to an aspirator, by means of which 36 gallons of air were made to pass through them. Into one of these tubes was introduced a piece of Schönbein's paper, which, before the experiment was ended, had become sensibly blue; in the other was placed a slip of blue litmus paper, which, during the whole course of the experiment, was not in the slightest degree reddened, as would have been the case if an acid had been present. It, however, gradually became bleached, a well-known effect of the action of ozone upon vegetable colours.

Another similar aspirator was connected with a system of Liebig's tubes, containing a solution of nitrate of silver, which would be rendered cloudy by the slightest trace of chlorine or any of its compounds; but the liquor remained perfectly limpid and transparent to the last.

Even admitting, therefore, that some uncertainty may attach to the validity of Schönbein's paper as indicative of the presence of ozone, when not corroborated by other independent tests, I am at a loss to what other cause to attribute the change of colour occurring after exposure to the air, when proper care has been taken to exclude *direct sunlight,* which, as will be stated hereafter, seems alone sufficient to separate iodine from its combinations, so as to produce the blue colour with starch.

Now it may be taken for granted, that any principle generally diffused throughout the atmosphere must have some special uses assigned to it in the economy of nature, and hence it becomes a matter of interest to the meteorologist, as well as to the chemist, to register the relative proportion in which ozone presents itself at different times

and places, so as to determine, if possible, the conditions upon which its appearance depends.

I have, therefore, been induced for some time past to note down, at intervals of twelve hours, by reference to a scale of colours, the amount of ozone assumed to be present in the atmosphere, during the winter season at Torquay, and during the rest of the year at Oxford, registering in each case the direction of the wind at the time being, and other meteorological conditions.

The following summary of the results obtained during 1864, 1865, and 1866, at Torquay, may lead to some inferences as to the dependence of the amount of ozone upon the direction of the wind, at least at that part of England.

1864.

In February.

Total amount of ozone was 230
Average per day . . . . 8·2

Wind.	Days.	Ozone.	Average.
W.	5	70	14·0
S.W.	2	28	14·0
N.E.	7	65	9·3
S.	4	36	9·0
E.	3	22	7·3
N.W.	2	5	2·5
N.	2	4	2 0

In March.

Total in the month . . . . 295·0
Average per day . . . . 8·3

Wind.	Days.	Ozone.	Average.
S.	1	52	13·0
W.	11	122	11·0
S.W.	1	11	11·0
N.W.	2	16	8·0
E.	4	28	7·0
S.E.	1	6	6·0
N.E.	7	38	5·5
N.	4	22	5·5

1865.

In January.

Total of ozone 160
Average . . . . 5·1

Wind.	Days.	Ozone.	Average.
S.	1	10	10
W.	6	53	8·8
S.W.	1	6	6·0
S.E.	2	12	6·0
N.W.	8	44	5·5
N.	4	17	4·25
E.	5	12	2·40
N.E.	4	6	1·50

In February.

Total of ozone 266
Average . . . . 9·5

Wind.	Days.	Ozone.	Average.
E.	1	15	15·0
W.	7	91	13·0
S.	3	30	10·0
S.W.	7	66	9·4
N.W.	3	25	8·3
N.E.	4	32	8·0
S.E.	1	3	3·0
N.	2	4	2·0

In March.

Total of ozone 222
Average . . . . 7·16

Wind.	Days.	Ozone.	Average.
S.	2	25	12·5
W.	4	48	12·0
S.W.	2	19	9·5
N.E.	4	38	9·5
S.E.	3	22	7·3
N.W.	6	39	6·5
E.	3	18	6·0
N.	7	13	1·85

1866.

In January.

Total of ozone . . . . 220·0
Average . . . . 7·0

Wind.	Days.	Ozone.	Average.
N.E.	1	13	13
E.	1	12	12
S.W.	7	69	9·8
W.	7	59	8·4
N.W.	5	37	7·4
S.	2	13	6·5
S.E.	2	10	5·0
N.	6	7	1·1

In February.

Total of ozone 169
Average 6

Wind.	Days.	Ozone.	Average.
S.W.	5	47	9·4
W.	10	81	8·1
N.W.	6	37	6·1
N.	6	4	·6
N.E.	1	0	·0

In March.

Total of ozone 211
Average . . . . 6·4

Wind.	Days.	Ozone.	Average.
S.W.	1	15	15
S.	1	12	12
S.E.	4	48	12
N.W.	6	41	6·8
W.	8	54	6·7
E.	1	6	6·0
N.E.	3	12	4·0
N.	7	22	3·1

The following is a summary of the results :—

TABLE I.

Average Amount of Ozone during two months in 1864, three months in 1865, and three months in 1866, at Torquay.

	W.	S.W.	N.E.	S.	E.	N.W.	N.	S.E.
1864.								
February . . .	14·0	14·0	9·3	9	7·3	2.5	2	0
March . . .	11·0	11·0	5·5	13	7	8	5·5	6
1865.								
January . . .	8·8	6·0	1·5	10	2·4	5·5	4·25	6
February . . .	13·0	9·4	8·0	10	15	8·3	2·0	3·0
March	12·0	9·5	8·5	12·5	6	6·5	1·85	7·3
1866.								
January . . .	8·4	9·8	13·0	6·5	12·0	7·4	1·1	5·0
February . . .	8·1	9·4	0	0	0	6·1	0·6	0
March	6·7	15·0	4·0	12·0	6·0	6·8	3·1	12·0
Total . . .	82·0	84·1	49·8	73·0	55·7	51·1	20·4	39·3

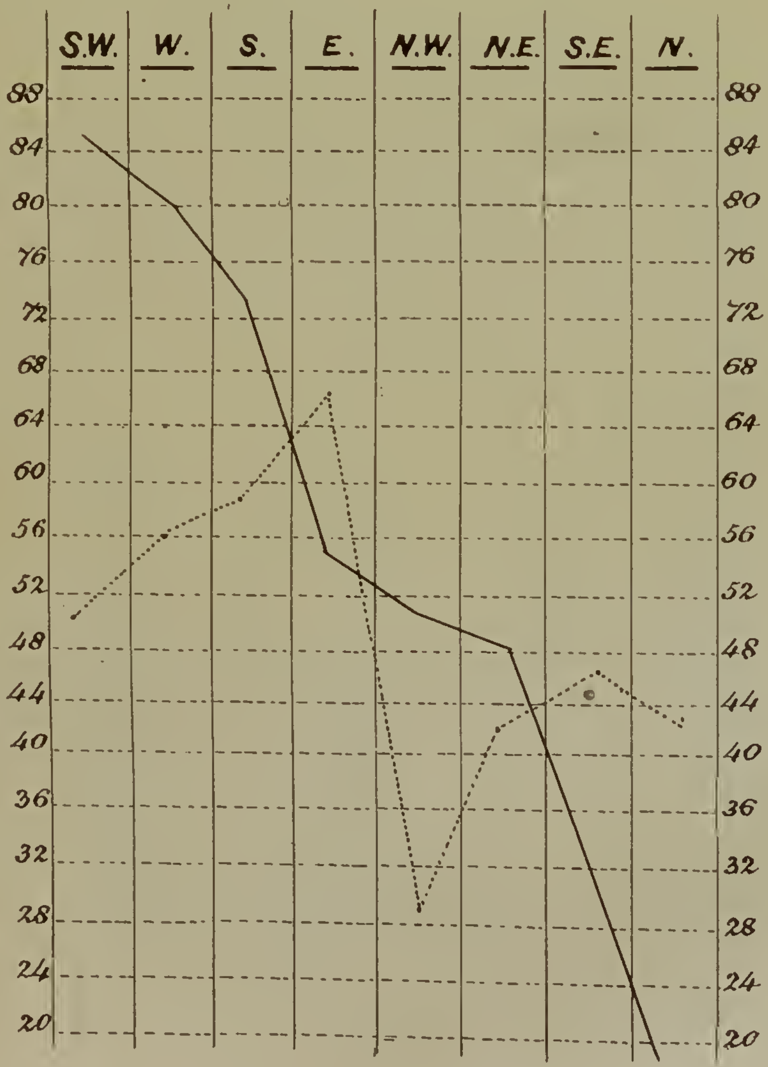
From this it would appear that in Devonshire there is, on an average, most ozone when the wind is blowing from the south-west and west, the numbers representing each of these nearly corresponding; that the next in efficacy as a carrier of ozone is the south; that the east, north-east, and north-west come lower still, but are nearly alike in this respect; that the south-east stands lower; and that the north is lowest of all, its quality as a carrier of ozone being to that of the south-west wind only as 20 to 84, or less than one-fourth.

But when I compared these observations with those undertaken at Oxford during the same period of time, although at a different season of the year, I found that the same law does not obtain.

From the table drawn up as the result of my ozone observations during the months of May, June, and August, 1865, and of April, May, June, and July, 1866, in which the average amount of ozone for each day during which the wind blew in a certain direction was noted, it appears that whilst the average amount of ozone per month at Torquay was 58·0, that at Oxford was only 43·3, and moreover, that whilst at the former place the largest amount occurred

during the prevalence of the south-west winds, and the least during that of the north; at Oxford, on the contrary, the east wind stands highest of all in its ozone-bearing properties, and the north-west lowest; whilst the difference between the respective winds in this respect did not exceed 5 to 2; instead of 4 to 1, as at Torquay.

In the following diagram the variations in the amount of ozone according to the direction of the winds are noted, those at Oxford being indicated by a dotted, those at Torquay by a continuous line, from which it appears that there is no correspondence in this respect between the two places.



In drawing conclusions, however, from the above premises, it must be confessed that some caution must be exercised; for it is evident that two causes might influence the results, and thereby render uncertain the quantitative estimation of the ozone by the method described.

The first of these is the influence exerted by light upon the coloration of the exposed paper, a point upon which I shall afterwards say more.

The absence, indeed, of any coloration of the paper for several days together, as shewn by the register, proves that the situation selected was sufficiently protected from strong light to prevent the paper from being much affected by this cause, for certainly the paper was sometimes least changed when the day was brightest.

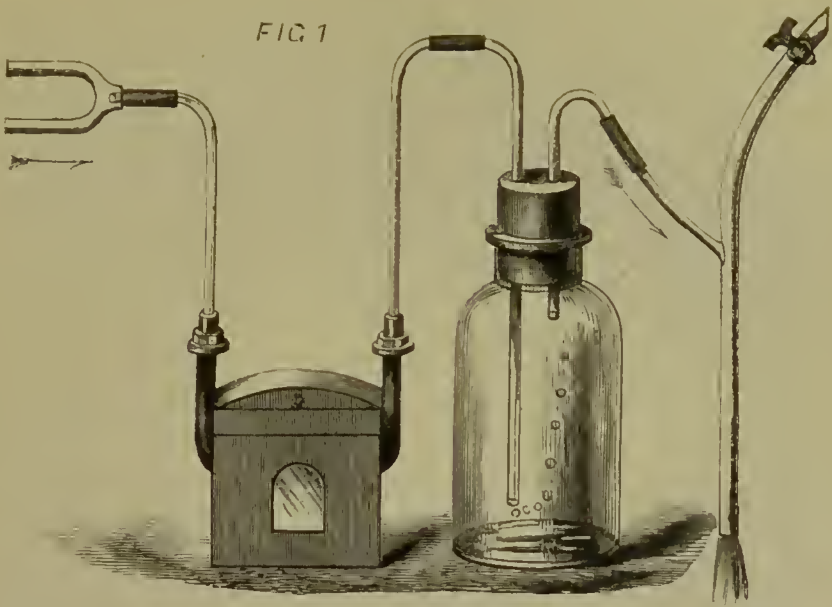
Still, however, a part of the result may have beeu due to this influence, as I have fouud a difference in the amount of coloration between paper placed in a tube blackened externally, and in one freely admitting diffused light.

Another source of uncertainty arises from the difference in the force of the wind at one time and another. It is evident that this circumstance must influence materially the amount of ozone which acts in a given time upon the paper exposed to its influence; and hence, as in the south-west of England some of our most violent gales proceed from that quarter, it may be said, that the greater amouut of coloratiou observed was due to the force of the wind, and not to its direction.

I have lately constructed an apparatus by means of which this cause of uncertainty may, I conceive, be eliminated, as the air is made to pass through the tube containing the ozone paper at a certain definite rate, as measured by a gas-meter in connexion with the aspirator beyond.

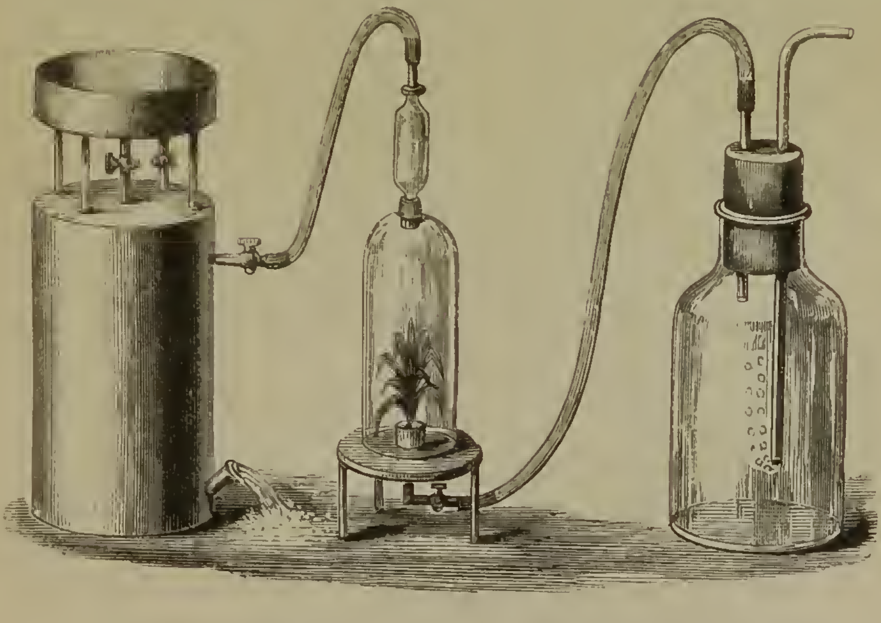
The apparatus has not been, as yet, employed sufficiently to allow of my reporting the results, but in the meantime I may remark, that although the objection stated might be apt to shake our confidence in any small number of observations, yet when the latter are extended over many months, and indicate a difference so great as that from 84 and 82 to 33 and 20 between the ozonizing influence of the west and

FIG. 1



(See p. 62.)

FIG. 2



(See p. 72.)

water impregnated with carbonic acid gas during sunshine; and I have myself made a number of experiments this summer bearing upon the same point, in none of which I was able to discover evidence of any ozone in the gas collected from leaves, although the proportion of oxygen present in it was sufficient to rekindle a red-hot match.

I also modified the experiment by placing tubes filled with common air over jars containing water, into which fresh leaves were introduced, and exposed them to the solar light, expecting that if any ozone were generated, it would become discoverable by diffusing itself through the air of the tube in connection with the jar.

But here I was also disappointed in obtaining satisfactory results, the only eases in which the paper was affected being those in which light had been allowed to come into contact with the air of the tube.

Now, I have found that the direct rays of the sun are capable, by themselves, of decomposing iodide of potassium, and thus of producing a blue tint on Schönbein's paper; and, moreover, that Moffat's becomes speedily changed under the same influence. Even diffuse light accelerates the action of ozone upon the paper, although it does not seem able by itself, within any limited time, to produce the chemical change, as it has frequently occurred to me, to find paper remain unaltered for days together after free exposure to the air in the shade.

The following table will shew that paper wholly protected from light is less affected than the same contained in a tube to which light was admitted, but still that even in the former case a certain influence was exerted by the ozone present.

TABLE II.

Experiments performed during the day, but in the shade.

	Tube blackened.			Not blackened.		
	Hours.			Hours.		
	4	8	12	4	8	12
July						
14	0	0	0	4	4	5
15	0	0	2	0	3	3
16	0	0	0	0	2	2
17	0	2	2	2	2	3
18	0	0	0	1	1	1
19	0	1	2	2	2	3
20	0	0	0	0	0	2
21	0	0	0	0	1	3
24	0	0	0	0	0	0
25	0	0	0	0	0	0
26	0	0	2	0	1	4
27	0	0	1	0	1	3
28	0	0	3	0	3	5
29	1	2	3	3	4	6
30	3	3	4	2	4	7
31	0	2	2	1	3	4
August						
1	1	1	1	3	3	4
2	0	0	2	1	2	3
3	0	1	3	0	2	4
4	0	2	3	2	3	5
5	0	1	1	1	2	3
6	0	2	3	1	4	6
7	1	2	4	2	3	6
8	2	3	4	3	4	6
9	0	0	0	0	0	0
10	1	1	4	3	4	5
11	1	3	4	4	5	7
12	0	0	2	0	3	4

It is, therefore, always necessary, when observations on the presence of ozone in air are taken, not only to provide that the paper should be well screened from direct light, but also that the amount of coloration due to that diffused should be allowed for, by comparing the tint produced under this condition with that obtained from the same paper placed in the dark.

By attending to these precautions, I think myself warranted in concluding, that ozone is really disengaged by the green parts of plants, for when I conducted the experiments in a different manner from that in which they were carried on by Dr. Gilbert, I produced a degree of coloration upon

Schönbein's papers which does not seem attributable to any other cause.

In the experiments now alluded to, the whole plant, or a portion of it, was covered over by a jar, having suspended in it one or more slips of Schönbein's paper in immediate juxtaposition, but not in actual contact with the plant, and a rapid circulation of air through the jar was prevented by attaching a piece of porous paper loosely to the open extremity of the vessel. The papers were examined at intervals of 1, 3, 6, and 9 hours, and the amount of coloration, if any, was recorded.

As the admission of a certain amount of solar light to the plant constituted the very condition of the experiment, it was necessary to eliminate the effect due to the action of this agent upon the paper, from that produced by the mere disengagement of ozone from the plant, and for this purpose a jar with a piece of Schönbein's paper suspended in it was exposed to the sun under the same conditions as in the former instance, except that no plant was introduced. At the same time a jar containing a similar slip of paper was placed near the others, protected from the sun's light by a dark shade, in which case it was inferred, that any coloration which might take place would simply be due to the ozone floating in the atmosphere, and thus that the effect attributable to the latter might be estimated and allowed for.

The following are a few of the general results deduced from the experiments I made, of which a tabular view is given :—

Table III.

Experiments upon the effects of the Leaves of Plants during sunshine upon Schönbein's Iodised Paper.

No.	Family.	Name of Plant.	Date.	Time of expoeure.	Degree of Coloration. When the paper was placed over the plant.	When the paper was apart from the plant. Expoeed to light.	When the paper was apart from the plant. In the dark.	Circumstances of the Weather, &c.
				Hours.				
1	Acanthaceæ .	Acanthus speciosus . . .	June 23rd	1	3	·3	0	Sun bright. Wind E. Ozone in the open air in 24 hours = 4. Th. max. 72, min. 52.
				3	7	3	1	
2	Araliaceæ . .	Aralia racemosa	June 28th	3	2	0	0	Sun bright. Wind N. Ozone = 12. Th. max. 78, min. 50.
				6	8	4	2	
3	Aristolochiaceæ	Aristolochia clematitis .	May 28th	6	4	..	..	Day cloudy. Wind W.; high. Oz. 11. Th. max. 67, min. 42.
4	Balsamineæ .	Impatiens parviflora . .	June 16th	1	4	1	0	Sun bright. Wind W.; high. Oz. 0. Th. max. 61, min. 50.
5	Berberideæ .	Berberis iberica	June 29th	3	0	2	0	Sun bright. Wind N.; high. Oz. 8. Th. max. 77, min. 60.
				6	2	3	1	
6	Boragineæ .	Symphytum asperrimum .	June 26th	1	1	0	0	Day fine and bright. Wind N.E. to E. Oz. 10. Th. max. 76, min. 57.
				3	3	2	0	
				6	4	4	1	
7	Campanulaceæ .	Campanula alternifolia .	June 4th	1	3	1	0	Day cloudy; warm. Wind S.E. to W. Oz. 5. Th. max. 68, min. 57.
8	. .	Phyteuma campanuloides			3	1	0	
9	Caprifoliaceæ .	Symphoria racemosa . .	June 30th	3	3	1	0	Day fine. Wind S. Oz. 10. Th. max. 77, min. 50.
				6	3	4	1	
10	Caryophylleæ .	Saponaria officinalis . .	June 22nd	1	5	3	0	Day fine. Wind N.W. Oz. 3. Th. max. 70, min. 57.
11	Cornaceæ .	Cornus alba	June 29th	3	2	2	0	See No. 5.
				6	2	3	1	

TABLE III.—*continued.*

No.	Family.	Name of Plant.	Date.	Time of Exposure.	Degree of Coloration. When the paper was placed over the plant.	Degree of Coloration. When the paper was apart from the plant. Exposed to light.	Degree of Coloration. When the paper was apart from the plant. In the dark.	Circumstances of the Weather, &c.
				Hours.				
12	Cruciferæ . .	Lepidium latifolium . .	May 28th	6	8	..	..	See No. 3.
13	Compositæ . .	Balsamita vulgaris . . .	May 29th	6	5	2	1	Day fine; cloudy. Wind N. Oz. 3. Th. max. 62, min. 45.
14	. .	Cacalia suaveolens . . .	May 31st	4	3	3	0	Day fine. Wind E. Oz. 10. Th. max. 64, min. 49.
				8	2	3	1	
			June 2nd	1	2	1	0	Sun bright; thunder showers. Wind E. Oz. 8. Th. max. 69, min. 51.
				3	2	2	1	
				6	4	3	0	
				9	4	2	0	
15	. .	Calendula officinalis . .	June 16th	1	3	1	0	See No. 4.
16	. .	Hieracium humile . . .	May 28th	6	4	1	0	See No. 3.
			May 31st	4	0	3	0	See No. 14.
17				8	5	3	1	
18	. .	„ rupestre . . .	May 29th	6	5	2	1	See No. 13.
19	. .	Rudbeckia subtomentosa .	May 29th	6	7	2	1	Ditto.
	. .	Solidago elliptica . . .	May 29th	6	6	2	1	Ditto.
			May 31st	4	4	3	0	See No. 14.
				8	6	3	1	
20	. .	„ latifolia . . .	May 31st	4	3	3	0	Ditto.
				8	7	3	1	
21	Coniferæ . . .	Pinus excelsa	May 29th	6	3	2	1	See No. 13.
			May 31st	4	0	3	0	See No. 14.
				8	3	3	1	
			June 2nd	1	4	1	0	See No. 15.
				3	4	2	1	
				6	3	3	0	
				9	3	2	0	

				6	4	4	2	
23	Dipsaceæ . .	Scabiosa sylvatica . . .	June 28th	3	4	0	0	See No. 2.
				6	4	4	2	
24	Eleagneæ . .	Eleagnus angustifolia . .	June 29th	3	4	2	0	See No. 5.
				6	5	3	1	
25	Ericaceæ . .	Rhododendron positicum .	June 23rd	1	1	3	0	See No. 1.
				3	1	4	0	
26	Gentianeæ .	Pneumonanthe acaulis .	June 30th	3	2	1	0	See No. 9.
				6	2	..	1	
27	Gramineæ .	Hierochloe borealis . . .	June 26th	1	1	0	0	See No. 6.
				3	3	2	0	
				6	3	4	1	
28	Grossularieæ .	Ribes sanguinea . . .	June 28th	3	0	0	0	See No. 2.
				6	3	4	2	
29	Labiatæ . .	Mentha crispa	June 4th	1	2	1	0	See No. 7.
30	Leguminosæ	Ulex europæus	May 28th	6	6	..	..	See No. 3.
31	Lobeliaceæ .	Lobelia tupa	June 30th	3	4	1	0	See No. 9.
				6	6	4	1	
32	Lythrarieæ .	Lythrum virgatum . . .	June 23rd	1	3	3	0	See No. 1.
				3	6	4	0	
33	Malvaceæ . .	Althæa rosea	June 16th	1	..	1	0	See No. 4.
34	. .	Lavatera olbia	June 26th	1	1	0	0	See No. 6.
				3	3	2	0	
				6	4	4	1	
35	Nyctagineæ .	Oxybathus violaceus . .	June 23rd	1	3	3	0	See No. 1.
				3	3	4	0	
36	Oleaceæ . .	Ligustrum vulgare . . .	June 26th	1	0	0	0	See No. 6.
				3	2	2	0	
				6	4	4	1	
37	Onagrariæ .	Chamœnerion rosmarini-folium	June 23rd	1	3	3	1	See No. 1.
				3	4	4	0	
38	Papaveraceæ	Papaver somniferum . .	June 22nd	1	1	3	0	See No. 10.
39	Plumbagineæ	Plumbago europæa . . .	June 22nd	1	6	2	0	See No. 10.
				3	3	3	1	
40	Polemoniaceæ	Collomia grandiflora . .	June 16th	1	2	1	0	See No. 4.
41	Polygoneæ .	Polygonum divaricatum .	June 4th	1	3	1	0	See No. 7.
42		„ cymosum . .	Do.	1	2	1	0	

TABLE III.—*continued.*

No.	Family.	Name of Plant.	Date.	Time of exposure.	Degree of Coloration. When the paper was placed over the plant.	When the paper was apart from the plant. Exposed to light.	When the paper was apart from the plant. In the dark.	Circumstances of the Weather, &c.
				Hours.				
43	Ranunculaceæ	Helleborus fœtidus	June 26th	1	1	0	0	See No. 6.
				3	2	2	0	
				6	4	4	0	
44	Resedaceæ	Reseda luteola	June 22nd	1	2	3	0	See No. 10.
45	Rosaceæ	Potentilla anserina	June 22nd	1	3	3	0	See No. 10.
46	Rutaceæ	Dictamnus fraxinella	June 23rd	1	1	3	0	See No. 1.
				3	6	4	0	
47	Salicineæ	Salix alba	June 30th	3	2	1	0	See No. 9.
				6	4	4	1	
48	Saxifrageæ	Astilba rivularis	June 28th	3	3	0	0	See No. 2.
				6	3	4	..	
49	Scrophularineæ	Phygelius capensis	June 23rd	1	2	3	0	See No. 1.
				3	4	4	0	
50	Simarubeæ	Alianthus glandulosa	June 30th	3	4	1	0	See No. 9.
				6	6	4	1	
51	Solaneæ	Physochlæna orientalis	June 23rd	1	1	3	0	See No. 1.
				3	3	4	0	
52	Tiliaceæ	Tilia europæa	June 29th	3	3	2	0	See No. 5.
				6	3	3	1	
53	Tropæoleæ	Tropæolum majus	June 16th	1	2	1	0	See No. 4.
54	Ulmaceæ	Ulmus montana	June 29th	3	1	2	0	See No. 5.
				6	5	3	1	
55	Umbelliferæ	Meum athamanticum	June 22nd	1	1	3	0	See No. 10.
56	Urticaceæ	Urtica canadensis	May 29th	6	5	2	1	See N. 13.
57	Violaceæ	Viola grandiflora	June 23rd	1	1	2	0	See No. 1.
				3	1	3	1	

From this table it will be seen that the experiments were tried upon fifty-seven species of plants belonging to forty-seven natural families.

In thirteen of these cases the coloration produced upon paper introduced into the jar containing the plant, was less in degree than that occasioned by light upon the same suspended in an empty jar for the same period.

But in thirty-two cases the reverse was observed, the degree of coloration attributable to the plant being sensibly greater.

In all, however, the coloration of paper placed in a darkened jar was much less considerable than in one freely exposed to light; indeed, in most instances, the paper remained uncoloured, and in none, even after six hours' exposure, did it exceed 2° of the scale.

The highest amount of coloration due to mere light was, after one hour's exposure, = 3; after six hours, = 4. This occurred in each case nine times.

Now in the jars containing a growing plant, the coloration of the paper in an hour amounted in one case to 5°; in two cases to 4°; in seven cases to 3°; in four cases to 2°; in four to 1°; and in three only to none at all.

In a few cases the coloration went on increasing from the first hour to the third, and even to the sixth; but never at any regular rate of progression. Thus *Solidago latifolia* in one hour produced a coloration = 4°; and in six hours one of 6°.

Hieracium rupestre in one hour produced 4°; in three hours, 5°; in six hours, 6° of coloration; whilst paper in a jar exposed to the same amount of sunlight, without access to a plant, was coloured in one hour 1°; in three hours, 2°; and in six hours, 3°.

On the other hand the leaves of *Pinus excelsa*, which in one hour gave a coloration to paper amounting to 4°, produced no deeper tint in three hours; and in six hours the colour of the paper had sunk to 3° of the scale, at which point indeed it remained after 9 hours' exposure.

Other anomalies of an equally striking character may be observed on consulting the tabulated results, and they seem

only explicable by considering, that owing to the volatile nature of iodine, a portion of that disengaged from its combination by the ozone present is constantly becoming dissipated, so that the amount of coloration is an *index*, not of the quantity of iodide of potassium decomposed during the time the experiment had been continued, but only of the ratio between the oxidizing action which sets the iodine free, and the tension of the latter, which causes it to evaporate into space.

Something may also be due to the bleaching effect which ozone exerts upon vegetable colours, owing to which the blue tint of the combination between the iodine and the starch may be discharged shortly after its formation; and a further source of uncertainty may arise from the presence of ozone in the atmosphere at the time being, which would influence the paper in the same manner as that disengaged from the plant.

With the view of eliminating, so far as possible, these several sources of error, and of determining what part of the effect observed was really due to the ozonizing influence of the leaves, I devised the following form of apparatus:—

A jar having a wide tube open at both ends ground into its tubulure, was placed over a growing plant, and rested upon a ground brass plate, to which it could be fixed nearly air tight.

A flexible tube screwed into the brass plate allowed air to enter the jar from without, and a similar one was attached to the upper end of the tube, communicating with an aspirator, by which air could be drawn continuously through the jar. The air that entered the jar was, however, deprived of ozone by being made to pass through a washing bottle containing iodide of potassium and a little starch, which by its coloration would indicate when the liquid was saturated; and moreover, before entering the jar, the air, after passing through the washing bottle, was made to come in contact with a slip of Schönbein's paper, placed in an intervening tube.

Similar slips were placed in the jar in near proximity to, but not in contact with the plant, as well as in the tube

above, a part of which was exposed to light, whilst the rest was covered with black paper for the purpose of excluding it.

After several hours' exposure to light, and after passing through it from 24 to 30 gallons of air, the plant was removed, and the papers examined.

The following were the results obtained in five experiments conducted in this manner:—

1st. Experiment with a small plant of *Veronica salicifolia.*

Duration of the experiment.	Air passed through the jar.	Paper.		
		In the jar.	In the upper tube.	
			Part exposed to light.	Part darkened.
10 hours.	27 gall.	Coloured to 5 of scale.	·4	3

I should infer from this that sufficient ozone had been generated by the plant to produce a coloration equal to 3°.

2nd. Experiment on a variety of Cape Pelargonium.

Duration of the experiment.	Air passed through the jar.	Paper.		
		In the jar.	In tube exposed to light.	In tube when darkened.
10 hours.	30 gall.	3	3	1

The day was overcast, and therefore less favourable to the experiment than the preceding one had been.

3rd. Experiment with *Chrysanthemum sinense.*

Duration of the experiment.	Air passed through the jar.	Paper.		
		In the jar.	In tube exposed to light.	In tube when darkened.
10 hours	30 gall.	2	2	1

Here, therefore, the paper placed in the darkened portion of the tube would seem to indicate the amount of ozone due to the plant.

Experiment 4, with *Phytolacca decandra.*

Duration of the experiment.	Air passed through the jar.	Paper.		
		In the jar.	In tube exposed to light.	When darkened.
8 hours.	30 gall.	Owing to the paper being placed so close to the plant as to be wetted by its moisture, no coloration.	4	2

Here, although the accident pointed out prevented the paper in the jar from shewing any effect, there would seem to have been ozone enough generated to produce a coloration of 2° in the darkened portion of the tube above.

Experiment 5, on a Convolvulus.

Duration of the experiment.	Air passed through the jar.	Paper.		
		In the jar.	In tube.	
			Exposed to light.	Darkened.
9 hours.	21 gall.	5	5	2

Upon the whole, then, I felt myself warranted in concluding that, inasmuch as, although ozone had been excluded by the method described, the paper introduced into the tube placed over the plant was in every instance affected, the quantity of this ingredient generated by the plant itself might be estimated by the degree of coloration, which had taken place in the paper placed in the darkened portion of the tube.

After the above experiments had been in a great degree completed, I found, by turning to the 18th volume of the 4th series of the *Annales des Sciences Naturelles,* that I had been anticipated in the conclusions arrived at by M. Kosmann, of Strasburg, who, from a series of not less than 253 observations, had felt himself warranted in inferring, that Schönbein's papers placed over growing plants are more deeply affected in a given time, than others left in contact with the open air at a distance from all vegetation.

As, however, this gentleman seems to have adopted no further precaution than that of freely suspending the paper,

in the open air, over the plants in the one instance, and at a distance from them in the other, it may not be thought, that the experiments which I have detailed, in which steps were taken to prevent any ozone from reaching the paper, except from the plant itself, are altogether thrown away with reference to the establishment of the conclusion arrived at.

With regard to the absence of any coloration, when paper was exposed to the action of the gas given off by leaves immersed in water, an explanation may be found in the readiness with which ozone is destroyed by any kind of organic matter.

The latter would doubtless exist floating in water in which leaves had been immersed, and would thus prevent any ozone generated by the plant from rising into the tube fixed into the tubulure of the jar containing leaves. Indeed, in several experiments, which I do not detail, as their results were vitiated by this cause, I found that the mere transmission of the ozone through a caoutchouc tube was sufficient to cause its disappearance.

The want of any definite relation between the intensity of colour and the time of exposure, may be referred to the volatility of the iodine, and its consequent escape, so that it might easily happen, that if the evolution of ozone did not proceed at an uniform rate, the coloration of the paper would become less instead of greater during the latter hours of the experiment. Indeed I have found in some cases, that the paper returned, after a time, almost to its original whiteness, and moreover, on days when ozone was absent from the atmosphere, that iodized paper, rendered blue by a weak acid, would lose its colour on exposure to the air.

It must be confessed that the want of any definite rate of progression in the deepness of the tint produced upon Schönbein's paper, in proportion to the number of hours during which it had been exposed, is calculated to shake one's confidence in this test, considered as a measure of quantity. Thus the following table will represent the indications afforded during the month of July last, by paper exposed in the open air during 4, 8, and 12 consecutive hours.

TABLE IV.

Degree of Coloration produced on Schönbein's Paper on the days mentioned.

	Hours.		
	4	8	12
July 14	3	4	6
„ 15	3	3	6
„ 16	2	2	2
„ 17	2	3	3
„ 18	1	1	2
„ 19	3	3	3
„ 20	2	2	2
„ 21	1	1	4
„ 22	0	1	1
„ 23	0	0	0
„ 24	1	3	4
„ 25	1	2	5
„ 26	1	3	6
„ 27	3	5	7
„ 28	3	6	9
„ 29	4	4	5
„ 30	3	4	6
„ 31	1	2	4
August 1	3	3	4
„ 2	3	5	6
„ 3	3	3	4
„ 4	3	5	8
„ 5	3	4	8
„ 6	4	6	7
„ 7	0	0	2
„ 8	4	4	6
„ 9	4	6	8
„ 10	3	4	6

I have seen it somewhere asserted that flowers as well as leaves emit ozone, but this statement is entirely opposed, both to M. Kosmann's observations, and to my own.

In one or two cases, indeed, a certain degree of coloration was observed, but this may be referred to other causes, as in the case of the Fraxinella, to the essential oil emitted by the flower, which may have possibly affected the paper. In the majority of instances at least, the degree of coloration produced was not greater than what took place in the same paper by exposure to an equal amount of solar light where no plant was present. Considering, indeed, that the function of the flower in the vegetable economy is antagonistic to that of the leaf, the one assimilating oxygen, the other causing its emission, it could not be expected, that because

ozone is generated by the organs designed for nutrition, it should therefore be emitted by those destined for reproduction.

Indeed leaves do not appear to give out ozone in the dark, or when their office of emitting oxygen is suspended, a difference which can be easily accounted for, if we consider that at these times, as I long ago pointed out in my memoir "On the Action of Light upon Plants," in the Phil. Trans. for 1836, they contribute, like the other parts of a vegetable, towards the production of carbonic acid, although not in such a degree as to compensate for the amount of this gas which they decompose during the continuance of sunlight.

May not this serve to account for the injurious effect upon the health ascribed to flowers when placed in large quantities in sleeping apartments?

This has been ascribed by some to the oxygen they consume, but their influence in this way must be wholly inappreciable. It is possible, however, that the essential oils which some flowers exhale exert a noxious effect upon the nervous system; but independently of this, they may, perhaps, render the air of inhabited places less salubrious, by co-operating with other influences which contribute under such circumstances towards the destruction of ozone.

And this leads me to the last question to be considered, namely, the uses which ozone subserves in the economy of nature.

When we consider its remarkable oxidizing properties and the rapidity with which any organic matter, dead or living, undergoes a slow combustion in its presence, it seems reasonable to conclude, that this principle is an important agent for destroying putrescent animal and vegetable matter by oxidation, and thus for restoring to the atmosphere its purity.

Plants, indeed, decompose carbonic acid, and thus serve to maintain unaltered the original composition of the atmosphere; but the various products of putrefaction, the emanations from animals, living as well as dead, the exhalations from swamps and marshes, would still continue to con-

taminate it, were it not for the presence of some agent capable of removing these bodies from the fluid in which they are floating, and of rendering them innocuous by oxidation.

And that such a function is really discharged by ozone may be suspected, from finding traces of this ingredient in air which has blown from the sea, more generally than in that which has swept over an inhabited region, as has been already pointed out; also from recognising it in greater abundance in the air of the country than in that of towns, and from observing that it is entirely deficient in that of inhabited rooms, even at times when it can be clearly detected outside the dwelling.

Hence it appears not improbable that the salubrity of a place depends upon the presence of ozone, which may be regarded as an *index,* at once of the absence of organic matter in the atmosphere of the place, and of the readiness with which it would be removed whenever it chanced to be evolved.

And if the observations I have brought forward, on my own authority and that of others, be regarded as sufficient to establish the fact, that the green parts of plants during the day are constantly evolving ozone, they will tend to shew, that vegetation is not only, as Dr. Priestley originally pointed out, the appointed means for restoring the equilibrium of the atmosphere, but likewise the agent for destroying those impurities, which would otherwise make it unfit for the existence of the higher classes of animals, even although the proper proportion of oxygen in it were fully maintained.

It may be objected to this view, that ozone is said to be more abundant by night than by day, and in cold weather than in hot; but it must be recollected, that if light and heat stimulate those functions of plants by which it is generated, they likewise render more active those chemical processes by which new combinations are formed out of the constituents of decaying animal and vegetable matter, and that the ozone produced by vegetation or other causes is not confined to the spot where it is generated, but be-

comes speedily diffused through the general body of the atmosphere.

It must be regretted, indeed, that no better and more precise mode of estimating such minute quantities of ozone has been discovered, than that of the blue tint produced upon paper moistened with a solution of starch and iodide of potassium, or the brown colour induced upon proto-sulphate of manganese, as in the case of that proposed by Moffat. I cannot rely upon different samples of either paper yielding, under the same circumstances, exactly similar results, and, therefore, am loth to confide in their indications as furnishing corresponding measurements.

Nevertheless, by accumulating a number of observations carried on by the same person and under the same circumstances, and by eliminating, as I have endeavoured to do, the action due to the presence of light, I trust to having arrived at some conclusions, which may at once substantiate the reality of ozone existing as an important agent in nature, and also serve to point out one at least of the agencies by which it is supplied, as fast as it is consumed in the atmosphere, in the quantities required for the necessities of animal existence.

One of the first memoirs that I ever communicated to a scientific society was that published in the "Philosophical Transactions for 1836," in which I confirmed the original conclusions of Priestley, which had been disputed by Ellis and others, with respect to the purifying influence which plants exert upon the air, by restoring to it more oxygen by day than they consume by night, and in which I also determined the description of solar rays, which was most instrumental for that purpose—a point which, as M. Decandolle remarked in his late address to the Botanical Congress, is now regarded as having been fully substantiated by the later researches of Draper and Sachs, although I perceive it still seems to be disputed by certain philosophers. Should I now have succeeded in establishing to the satisfaction of the scientific world, that these same green parts of plants, at the very time that they are emitting oxygen, convert a portion of it into ozone, I might hope that these researches of

my later years will serve appropriately to wind up those undertaken in my younger ones, by shewing that vegetable life acts as the appointed instrument for counteracting the injurious effects of the animal creation upon the air we breathe, not merely by restoring to it the oxygen which the latter had consumed, but also by removing, through the agency of the ozone it generates, those noxious effluvia, which are engendered by the various processes of putrefaction and decay.

However, it may be urged by some, that it is premature to speculate as to the uses of a principle, until we are fully assured of its existence; and although it would seem extremely unaccountable, that air which had been in contact with a growing plant should, when introduced into a darkened tube, affect Schönbein's paper in the manner described, unless ozone were present in it, still it would be more satisfactory, if some other test, not liable to be affected by light, could be appealed to in confirmation of its presence. And if the suggestion of some new and improved method of determining ozone were to flow from the reading of this paper, I should deem myself amply repaid for the labour it has cost me, even though the conclusions I have sought to deduce should eventually be overthrown by other more precise methods of observation.

It is time, indeed, that the question as to the existence of ozone in the atmosphere should once for all be set at rest; for at present, whilst it is disputed by some eminent chemists, it is taken for granted by meteorologists, and thus the weather tables constructed in this and most other countries are swelled by an additional column devoted to ozone observations, which, if fallacious, should be swept away as fostering the popular belief in a non-entity; and if real, should be turned to some account by those, whose business it is to investigate the unexplored properties of the atmosphere in which we live.

On the Influence of Carbonic Acid Gas on the Health of Plants, especially of those allied to the Fossil Remains found in the Coal Formation.

"AT the Meeting of the British Association for the Advancement of Science held at Oxford in 1847, it was resolved, that a Committee, consisting of Sir H. T. De la Beche, Sir W. J. Hooker, Dr. Daubeny, Dr. J. D. Hooker, Mr. A. Henfrey, and Mr. R. Hunt, be requested to investigate the influence of carbonic acid on the growth of plants allied to those found in the coal formation."

This investigation was accordingly entered upon by myself in the spring of 1848, by means of an apparatus consisting of two jars of corresponding size, each containing about 2,800 cubic inches of air, the edges of which rested upon a smooth slate table, having two circular holes perforated in it, into each of which a pan or pot containing the plants to be experimented upon was inserted.

By the aid of this apparatus I carried on a series of experiments both on flowering plants and on ferns, from which I inferred that the one as well as the other would continue for a fortnight at least unaffected by a dose of carbonic acid, bearing a proportion to the whole volume of air equal to from 5 to 10 per cent., but that 20 per cent. would prove injurious to the one, as well as to the other, in the course of two or three days. These results were however not offered to the Association at Swansea with any confidence, because the apparatus contrived for the purpose of carrying them on turned out to be defective, the difficulty of cementing the vessels containing the plants to the slate table, so as to render the apparatus impervious to air, being such, that

a large supply of gas was each day found requisite, in order to keep up the per-centage to the intended amount. Hence it was probable that during a portion of the time the real quantity of carbonic acid in the jar might have fallen very short of that with which it was proposed to operate.

I therefore renewed the experiments in the spring and summer of the present year (1849), in two ways, either of which had been ascertained by previous trials to preclude in a great degree the danger of leakage, and thus to render the amount of carbonic acid present whilst the experiment was being carried on, tolerably constant.

The first was that of allowing the jars, the edges of which had been well ground, to rest upon the surface of a solid and smooth slate table, greased along the line of its contact with the glass; the other to make them dip into shallow iron dishes with double rims, containing water to the depth of an inch, so that the air of the jar might be cut off from the external atmosphere. In neither of these cases was there a sufficient loss of gas to interfere with the results; in the former, the transmission of air between the smooth surfaces of the slate table and the jar being inconsiderable, and in the latter, the quantity of gas carried off by solution in the water being much reduced when the latter was covered with a thin pellicle of oil. Whatever indeed might be the loss in either case sustained, I took care to supply it, by introducing the requisite quantity once every twenty-four hours into the jars which contained the plants.

I am therefore now able to offer to the Association, with rather greater confidence than before, the following results, as confirmatory of those which were stated verbally in my Report, but which, for the reasons already assigned, were not published in the Transactions of the Association for last year.

May 14th.—In the first experiment, five healthy ferns, named *Nephrodium molle*, *Adiantum cuneatum*, *Gymnogramma chrysophylla*, and two species of *Pteris*, viz., *longifolia* and *serrulata*, were introduced into jar 1 standing in water, and a quantity of carbonic acid gas was admitted, which equalled 5 per cent. of the whole amount of air present in the jar.

No perceptible change occurring, the quantity was increased on the 17th to 10 per cent., and this amount was maintained, as nearly as possible to the same point, by occasional additions of the gas, till May 27th.

At the expiration of ten days there was no perceptible difference in the appearance of the ferns, either with reference to their preceding condition, or by comparison with that of five similar ferns, which had been kept for the same time under the corresponding glass, without any admixture of carbonic acid gas. The experiment was then continued till June 21st, so that the plants were exposed to the influence of carbonic acid gas in all for a period of thirty-three days, besides being subjected for seven days to about 5 per cent. of the same. At the end of this time only two of the ferns appeared at all damaged, namely, *Pteris longifolia* and *Nephrodium molle*, the fronds of both which were rather discoloured, those of the other three species remaining as before.

The same description of experiment was made upon a species of Pelargonium, which after having been during twenty-seven days exposed to the action of 10 per cent. of carbonic acid contained in the air of a large jar, appeared in exactly the same condition as a corresponding one placed under glass in a vessel free from any abnormal mixture of that ingredient.

From these, and from the experiments of the preceding year, it might be inferred that plants in general are tolerant of a much larger volume of carbonic acid gas than exists in the atmosphere at present; but it did not therefore follow, that the amount of carbonic acid decomposed, and of oxygen exhaled, would bear any proportion to the quantity with which their leaves were brought into contact.

From several trials indeed which I made as to the percentage of oxygen present in the jar at different stages of the experiment, I was led to infer that the amount of the latter was not increased in the degree which might have been expected; but, as a more easy way of determining the same point, I introduced a certain number of fresh leaves of an Helianthus, in each case exposing exactly the same

amount of surface, into jars filled with water containing different proportions of carbonic acid gas. In No. 1, for instance, the proportion of gas to water was only as 1 to 12; in No. 2 as 1 to 6; and in No. 3 as 1 to 3. Now it was found, that, instead of the oxygen disengaged by the leaves keeping pace with the supply of carbonic acid, only 0·7 of a cubic inch was given off from No. 3, whilst No. 2 had disengaged 4 cubic inches, and No. 1 3·3 cubic inches; and in another experiment only 0·1 was emitted by No. 1; 4·5 by No. 2; and 2·0 by No. 3, the other circumstances, as to time, exposure to light, &c., being in all cases the same. If therefore the disengagement of oxygen from leaves be, as is generally admitted, the result of their vital action upon the carbonic acid in contact, under the stimulus of light, it would follow, that where the carbonic acid exceeds a certain amount, that action is in a great degree suspended.

There is, however, an experiment of Count Rumford's, originally reported in the Philosophical Transactions for 1786, and alluded to by one of my co-adjutors in these investigations, I mean Mr. Hunt, in his late work entitled "The Poetry of Science," which would seem to imply that the decomposition of carbonic acid by plants was not a vital phenomenon, and consequently could not be influenced by any such circumstance as the application of a superabundant portion of this gas to the surfaces of their leaves.

Count Rumford states, that the property of causing water to emit oxygen in the sun, is possessed, not only by living plants, but likewise by threads of silk, by wool, and even by spun glass; in which case the decomposition of carbonic acid would seem to be simply the effect of light, the plant merely serving, by the surfaces it exposes to the water, to disengage from it, more rapidly than would otherwise happen, that oxygen which had been obtained without its direct agency.

On repeating this experiment, however, I found, as might have been anticipated, that at first no such effect took place when wool, cotton, silk, or spun glass were introduced into the water, but that after some days it occurred abundantly in every one of these cases—the disengagement of the gas,

however, being always coincident with the appearance in the liquid of green confervæ, to the action of which doubtless this decomposition of carbonic acid was to be attributed.

Accordingly the process went on, whether fibrous substances were placed in the water or not, although in the latter case somewhat less rapidly, the presence of such bodies serving to disentangle the particles of gas from their adhesion to the water more easily than would happen otherwise.

There cannot therefore be a doubt, that the common opinion, which regards the emission of oxygen from the surfaces of leaves, whether placed in water or in air, as a *vital* phenomenon, is the correct one, and hence it is quite consistent with analogy, that, as we have already seen, some one proportion of carbonic acid in the air should be more favourable to the exercise of this function, than any other one more considerable in amount would prove.

I was therefore encouraged to proceed in my inquiry as to the quantity of carbonic acid contained in air, which was decidedly prejudicial to the health of ferns.

With that view, specimens of the same five species as before were selected for experiment, and these were placed under the jar, which contained about 2,800 cubic inches of air cut off from the external atmosphere by water. To this air 1 per cent. of carbonic acid was at first added, and a daily increase to the same amount in the quantity present was kept up, until the proportion reached 20 per cent. This same quantity was then maintained in the jar for twenty days by successive additions, to compensate for the ascertained amount of leakage, now found to be inconsiderable, and the appearance of the plants was from time to time examined and noted.

It was not till the 13th day that any sensible alteration for the worse was perceptible, when we observed, that in *Pteris longifolia* the fronds had become very brown; in *Nephrodium molle* and in *Gymnogramma chrysophylla* two or three of the lower fronds shewed signs of yellowness; that those of the *Adiantum* looked in general very sickly, but that *Pteris serrulata* did not appear injured. The experiment was however continued seventeen days longer,

when it was found, for the first time, that the amount of carbonic acid present in the jar, as ascertained in the usual way by potash, exceeded what had been added; proving more decisively than before, that decay had commenced. The plants were accordingly taken out, and the following notes respecting their condition were entered in the Minute-Book.

Pteris longifolia.—All the old fronds are now dead, but the vitality of the rhizoma is not destroyed, for young fronds are putting out, and appear at present to be healthy.

Pteris serrulata even now appears but slightly damaged, its fronds being only more yellow than is natural.

Nephrodium molle seems in the same condition nearly as *Pteris longifolia.*

Gymnogramma chrysophylla.—Its old fronds slightly damaged and yellow, but young ones are putting out.

Adiantum cuneatum.—All the fronds have died down.

Thus it appears that this large amount of carbonic acid, even if gradually added, would in time prove fatal to plants of the above description, although operating upon them with various degrees of intensity, and apparently not exerting any specific influence upon the stem and roots.

That the effect however was attributable, not to the diminution in the proportion of oxygen consequent upon the addition of so large an amount of carbonic acid, but to something positively deleterious in the latter gas itself, was inferred, by exposing the plants to air impregnated with 20 per cent. of hydrogen, which in the course of ten days appeared to exert no sensible influence upon their health.

There did not appear to be any *very material* difference in the action of carbonic acid upon plants, whether it were suddenly or gradually introduced; for when I exposed the same ferns to air into which 20 per cent. of carbonic acid had been added all at once, it was not till the 9th day that any change in their appearance was perceptible, and then only in three of the specimens; *Pteris serrulata* and *Adiantum cuneatum* being scarcely, if at all affected.

However, on the 16th day the influence of the gas was manifest upon all except *Pteris serrulata;* the per-centage of carbonic acid was found to exceed that which had been

added from without, and the condition of the ferns generally was rather more unhealthy and faded than it had been in the foregoing experiment, where the gas had been added in successive doses[a].

So much for this part of the investigation, which seems to be in a manner prefatory to the one which may be regarded as the more immediate object aimed at by the Association in suggesting these researches, that being, whether a larger amount of carbonic acid than is present in our atmosphere would increase the vigour, and stimulate the growth, of the tribes of plants which are most connected with the fossil remains found in the coal formation.

With reference to this latter question, I am not so far advanced towards its determination as might have been desired.

During the last five weeks Ferns and Lycopodiums have been living in an atmosphere containing constantly 5 per cent. of carbonic acid, whilst corresponding specimens have been placed under similar circumstances, except that the abnormal amount of carbonic acid above stated was absent from the air of the jar. In both instances the Lycopodiums continue up to this time in perfect health, but it must be confessed that the *Adiantum cuneatum* and *flagelliforme*, which have been subjected to carbonic acid, appear less thriving than the corresponding plants not so treated.

It must be remarked moreover, that the per-centage of gas within the former jar has been increased to 5½ per cent., the additional ½ per cent. being attributable to the diseased state of some of the fronds.

The experiment however shall be continued for a longer period until more decisive results have been arrived at.

But supposing it to be ascertained that ferns will exist in air containing 5 per cent. of carbonic acid, it still remained a question, whether the animals that lived at the same

[a] I do not find that ferns suffer from confinement in large jars; and at all events, as the circumstances were precisely the same in the two cases, with the exception of the presence or absence of this excess of carbonic acid, the difference in the appearance of the specimens seems clearly referable to the latter cause alone.

period could have resisted the poisonous influence of so large a proportion of this gas.

In the coal formation, properly so called, Mollusks and Fish appear to be the animal remains principally detected, and the difference between the structure of existing species, and of those which were in being at so remote a period as the one alluded to, may be urged, as an objection to the idea of extending to the latter any inferences that might be deduced from experiments instituted upon the former.

Nevertheless as in so fundamental a function as that of respiration, a similar law pervades all the individuals belonging to the same great natural group at the present time, as for instance, what is true in this respect concerning the lowest in the scale of Mammalia, holds good likewise with certain modifications with regard to the highest, it may not be illogical to presume, that the difference as to time would not create any radical change in the relations of a particular class of animals to carbonic acid, and in their susceptibility to its influence.

With reference to the proportion of carbonic acid which water would abstract from air containing diffused through it so large an amount as 5 per cent. of this gas, the principles upon which such a problem may be determined have been long ago clearly laid down by Dr. Dalton.

As neither carbonic acid, oxygen, or nitrogen are retained in water by virtue of any chemical affinity, but simply in the ratio of their respective elasticities, it follows that the quantity of these gases present in it will be regulated by the amount of each existing at the time in the superincumbent air.

We know by experiment, that water would retain nearly about its own volume of carbonic acid; 0·65 of its volume of oxygen; and 0·42 of nitrogen; under the pressure of an atmosphere consisting wholly of the gas so retained.

If therefore we suppose the atmosphere in former times to have consisted of carbonic acid at 5 per cent. and of common air, maintaining its present constitution, 95, that is, of—

Nitrogen	76
Oxygen	19
Carbonic acid	5

the quantity of each gas retained by a volume of water under such circumstances would be as follows:—

Nitrogen	·03192
Oxygen	·01235
Carbonic acid	·05300
	·09727 [b].

Water, therefore, under an atmosphere of this constitution would still contain nearly as much oxygen as it does at present, and not more than ·05, or $\frac{1}{20}$, of its volume of carbonic acid, so that the condition of the gas expelled from the water would be such, as to consist in 100 parts of—

Carbonic acid	54·5
Nitrogen	32·9
Oxygen	12·6
	100·0

Now I am enabled to prove, that a much larger proportion of carbonic acid than that supposed may exist in water without affecting the health of fish at the present time. On one occasion indeed I agitated some river water in a closed vessel with a mixture of common air and carbonic acid, in the proportion of 1300 of the former to 100 of the latter, or in an atmosphere containing 7 or 8 per cent. of carbonic acid, and found that a number of *Minnows* introduced into the water so impregnated died within twenty-four hours, although 29 cubic inches were found by experiment to have taken up only 1 cubic inch of carbonic acid, which is in the ratio of 2·5 per cent.

Nevertheless it was afterwards found by a number of experiments, that other fish, such as Perch and Roach, would live in water which contained from 5 to 10 per cent. of carbonic acid, the larger of which quantities would be nearly double that which has been shewn to be taken up by water

[b] As will appear by the following equation:—

Nitrogen	$·76 \times 042 = ·03192$
Oxygen	$·19 \times 065 = ·01235$
Carbonic acid	$·05 \times 1·06 = ·05300$
	·09727

under a pressure of 5 per cent. of the latter gas. On the other hand, where the quantity present might be estimated at 13 per cent. as compared to the volume of water, all the fish experimented upon speedily perished.

Nor was this merely the case with freshwater species, for I have had an opportunity within the last fortnight of repeating the same experiments at Ryde on certain sea-fish obtained off that coast. The species operated upon were those called Golden Maid (*Labrus*), two sorts of Pipe-fish (*Syngnathus*), Rock-fish (*Gobius niger*), Bull-fish (*Cottus scorpius*), and Flounder (*Platessa flesus*). Of these the Pipe-fishes and the Flounder remained alive for many hours in a tub of salt water containing 5 per cent. of carbonic acid, nor did they appear to suffer in consequence. When the amount was equal to 10 per cent., the Golden Maid (*Labrus*) was almost instantly affected, as were also the Pipe-fishes above operated upon.

Although, therefore, the difficulty of keeping sea-fish long alive in small quantities of salt-water, after they have been removed from their natural element, renders it more difficult to arrive at satisfactory results with them than with freshwater species, I think myself upon the whole warranted in concluding, that both kinds are equally tolerant of the smaller amount of carbonic acid, and alike susceptible of the poisonous influence of the larger.

Supposing however no error to exist in the calculation I have made above as to the amount of carbonic acid present in the water to which the minnows had been subjected, it will follow that whilst 5 per cent. is innoxious to some fish, 3 per cent. is noxious to others, and that the power of resisting its deleterious influence differs in different species. Nevertheless there seems reason for supposing, that an amount of carbonic acid in the atmosphere considerably larger than that which exists at present, would not communicate to the waters of the sea and rivers properties incompatible with the life of many fish.

Although reptiles are not supposed to have existed generally at so early a period as that of the carboniferous forma-

tion, yet as Saurians have been detected in the coal-beds of Greensburg in Pennsylvania, and in those of Saarbruck near Treves [c], which are regarded as belonging to the same epoch, and as they made their appearance so abundantly in that which comes next to it in point of antiquity, it appeared worth while to ascertain what power of resisting the influence of carbonic acid might be possessed by the tribes now in being which belong to the same class of animals.

With reference, however, to this department of the inquiry, the experiments hitherto made by myself are far from numerous: I have however found, that frogs introduced under a bell-glass containing 5 per cent. of carbonic acid gas, appeared not to suffer, although they were killed when its proportion amounted to 10 per cent. Similar results were also obtained in experimenting upon newts; so that it would seem as if, in accommodation to those arrangements of nature which were calculated to impart a greater luxuriance to the vegetation of the period alluded to, and to bring about during its continuance a larger accumulation of carboniferous matter, the lower tribes of animals, which at that time alone occupied the earth, were rendered less susceptible of the injurious influence of carbonic acid, than the higher orders subsequently created are found to be.

In conclusion, then, I may remark, that the general tenor of these experiments, so far as they have as yet gone, justifies us in inferring, that there is nothing in the organization of those plants and those animals of the present day, which appear most nearly allied to such as were in existence during the carboniferous epoch, or even somewhat subsequently to that period, militating against the probability, that a larger amount of carbonic acid may have been present in the atmosphere, and diffused through the waters of the sea and rivers, than is found, either in the one or in the other, at the present time; nor is there anything to prevent us from imagining, that the absorption of carbon by vegetables, and the consequent rapidity of their growth,

[c] See Lyell's "Travels in America," 2nd Series.

may, at least within certain limits, have borne some proportion to the greater amount of carbonic acid assumed to have been present at earlier periods in the history of our globe, although whether this be actually the case, is a point which I hope to be able hereafter to settle more to my satisfaction, as well as to report the results arrived at on some future occasion.

MEMOIRS ON THE SELECTIVE POWER OF PLANTS.

INTRODUCTORY REMARKS.

My first attempts to determine this question date from as early a period of my life as those already detailed relating to the "Action of Light upon Plants," for in the Linnæan Society's Transactions, vol. xvii., may be seen a Memoir bearing my name, the date of which is November 19, 1833, entitled "On the Degree of Selection exercised by Plants, with Regard to the Earthy Constituents presented to their Absorbing Surfaces," in which Memoir various experiments are reported tending to shew, that the earthy constituents which form the basis of the solid parts of a plant are determined as to *quality* by some primary law of nature, although the *amount* present depends upon the more or less abundant supply of the principles presented to them from without.

According to this principle, if an abnormal substance be brought into contact with their roots in the place of any one of the normal ingredients which the plant naturally contains, it is not absorbed, so that strontia, for example, cannot replace lime in the constitution of a plant.

This subject was again taken up by me at a later period with the advantage of more extended information, and the results were embodied in a Memoir read before the Chemical Society, and published in their Quarterly Journal, in vol. xiv. for 1862.

In this communication reference is made to the Memoir in the Linnæan Society's Transactions already alluded to, so that by perusing it, the reader will be placed in possession of the whole subject, so far as it is elucidated by my own researches, with the exception of certain experiments, which prove that plants are unable to form for themselves the mineral ingredients they contain, a fact at the present day regarded as self-evident, although at the time at which my first paper was published, the opposite conclusion was still entertained by some on the faith of the experiments of Schrader and others.

With reference, therefore, to the question which relates to the power possessed by plants of refusing to take up *abnormal* ingredients, I shall content myself with reprinting the paper contained in the Journal of the Chemical Society, vol. xiv., but before inserting it, shall make room for another communication of my own to the same Body, contained in vol. v. of their Journal, in which I have attempted to ascertain, how far the natural composition of a plant can be made to vary artificially, by substituting one *normal* ingredient for another, so as to alter the relative proportions in which they are usually offered.

On the Variation in the Relative Proportion of Potash and Soda present in certain samples of Barley grown in plots of Ground artificially impregnated with one or other of these Alkalies.

(*From the Quarterly Journal of the Chemical Society, Vol. V., 1853.*)

ALTHOUGH the necessity for a due supply of earthy and alkaline matters to the growing vegetable may be at present regarded as indisputable, a good deal still remains to be done before we can pretend to lay down, with any degree of certainty, the extent to which any one of those ingredients, commonly present in a particular plant, may be replaced by others, without affecting its health or development.

Saussure[a], indeed, had long ago ascertained, that a tree varied in its mineral constitution according to the nature of the soil in which it had grown; a fir, for instance, taken from a felspathic rock, being found by him to contain much potash, but no magnesia; whilst the same from a dolomitic rock was charged with magnesia, but exhibited a proportionate deficiency in alkali. Berthier[b], also, had afterwards confirmed this inference by his analysis of two samples of oak timber, the one obtained from Norway, the other from

[a] *Recherches sur la végétation*, 1804. [b] *Ann. Ch. Phys.* [2], lxxxii.

Allevard, in Dauphiny, as the proportion of soda and of potash was found by him greater in the former instance than in the latter.

The independent results of these trustworthy chemists, Liebig[c] has endeavoured to bring under the operation of a general law, which he has himself suggested, by shewing that the sum of the oxygen present in all the samples of timber analyzed, whether by Saussure or by Berthier, was uniformly the same, however much in relative amount the bases themselves might vary. This remarkable observation would certainly seem to favour the idea, that, provided there be bases present in the soil sufficient for neutralizing the organic or mineral acids generated or secreted by the growing vegetable, the nature of the former was comparatively unimportant; and thus might lead us to conjecture, that isomorphous bodies, at least, which replace each other in the structure of a mineral, might do the same also in the organization of a plant.

Amongst other facts tending to the same conclusion, may be cited the analyses made some years ago by Will and Fresenius, of barley grown in the interior of Germany, and in the more maritime districts of the Netherlands, as the former was found to contain a smaller proportion of soda in comparison with the potash present, than the latter.

But before we allow ourselves to push our inferences beyond the point to which the above facts strictly warrant us in proceeding, let us consider, for a moment, the arguments that may be adduced on the opposite side, as tending to shew, that a certain definite mineral composition is to be ascribed to each particular plant, and probably even to every one of the several organs of which it is made up. If the nature of the mineral ingredients present in a plant were a matter of indifference, provided only the neutralizing power possessed by them were sufficient for the purpose of enabling them to combine with the acids present; or even if, without proceeding to this extent, we suppose one alkali capable in general of supplying the place of another, or an alkaline earth of being substituted for an alkali, what then

[c] "Chemistry in its Application to Agriculture," &c.

is the end of that remarkable power of selection which all plants possess, and which is so remarkably evinced by those of marine origin, in their assimilation, not only of iodine, but also of potash, from a sea containing both in infinitesimal proportions only. The case of the Algæ, indeed, is only an extreme example of a generally pervading law; for it is sufficient to cast a glance over any series of vegetable analyses, as, for instance, the tabular view given by Dr. Karl Bischof in a late number of the *Journal für praktische Chemie*[d], of the composition of various plants, to be convinced, that they in general absorb potash in preference to, and even to the exclusion of, soda, without reference to the comparative abundance of the two alkalies in the soil.

Dr. Bischof shews, that out of a series of 200 ash analyses of land plants, or of particular portions of them, there are more than six-sevenths in which the proportion of potash was estimated to exceed that of soda, and more than one-fifth in which potash alone was found, these latter analyses moreover being the most recent, and probably, therefore, the most trustworthy. On the other hand, there is not a single plant known in which soda alone occurs, and only a few, excepting those belonging to the class of marine algæ, in which its amount exceeds that of potash. Bischof also assigns reasons for supposing that the proportion of soda in the ashes may often have been over-estimated. It seems, moreover, difficult to understand, why, if soda can take the place of potash, and lime or magnesia of either, the same should not be the case with strontia; and yet I found several years ago[e], that when a plant had been watered with a solution containing that earth, so minute a trace of it was found in its ashes, that it became impossible to suppose the principle in question to have entered into the composition of its tissue. Lastly, with reference to the results obtained by Will and Fresenius, I may be permitted perhaps to set against them some made a few years ago in my own laboratory, which tended to shew, that barley possessed the same mineral composition, so far as its alkalies were concerned,

[d] Vol. xlvii. [e] Linnean Trans., 1833.

whether it had been grown in the east or west coasts of England, or in the more central district of Oxfordshire[f].

Deeming, therefore, the subject to be one open to further investigation, I was induced last summer to institute a few experiments, the results of which I will next submit, as introductory to a statement of certain views, respecting the mode in which the two alkalies come to be found in the ashes of a plant, and the functions severally discharged by them in their living organization, which I am desirous of laying before this Society.

For the purpose of ascertaining how far the alkaline constitution of a crop of barley might be modified by a difference in the quality of the soil in which it was grown, I selected in the same part of the Oxford Botanic Garden, seven plots of ground, of equal size, and as nearly as possible similar in point of quality, exposure, &c. Two of these plots were manured with a pretty strong dressing of subcarbonate of potash; two with an equivalent amount of subcarbonate of soda; two with common salt, in quantity sufficient to supply the same proportion of soda as the last; whilst the seventh was left without any application at all. The size of the plots was such, that the quantity of potash added would be equivalent to about ten bushels of common salt to the acre, and to corresponding quantities of carbonate of soda, and of carbonate of potash to the same surface of soil[g].

In order to prevent these saline matters from being too speedily washed away by rain, I had previously dissolved them in water, and had mixed each of the several solutions with a bulk of stiff clay large enough to absorb the whole of the liquid. The mixture was then exposed to the sun and air, in order that the salts might become dry, and be

[f] Bakerian Lecture "On the Rotation of Crops," in the Ph. Trans. for 1845.

[g] The quantities being respectively:—

7 lbs. of pearl-ash,
20 „ of subcarbonate of soda in crystals,
9 „ of common salt to each plot,

these numbers representing equivalent amounts of the two alkalies.

thoroughly incorporated with the mass; after which the whole was thrown upon the plots intended for their reception, and mixed up intimately with the soil down to the depth of two feet from its surface.

An equal amount of barley was then sown in each of the said plots, and the crop obtained separately dried and weighed, the straw and grain having been kept distinct. A portion of each having been then incinerated, the soluble portion of the ash was taken up by water, after which everything, except the alkaline salts, was removed by the methods usually employed for that purpose. An aliquot portion of the alkaline residuum was in each case converted into chloride, and the potash separated from the soda-salt by means of the chloride of platinum.

The following were the results obtained by this method:—

Plots:

Manured with:	yielded: altogether.	yielded: of:	in 100 parts of the alkaline residuum. Potash.	Soda.
Carbonate of potash	average of the two crops 71½ lbs.	viz. grain 13½ lbs.	84·50	15·50
		straw 58 „	79·50	20·50
Carbonate of soda	„ „ 71 „	viz. grain 13½ „	76·50	23·50
		straw 57½ „	72·75	27·25
Chloride of sodium	„ „ 78¼ „	viz. grain 14½ „	76·50	23·50
		straw 63¾ „	76·50	23·50
Unmanured.	produce of the bed 63 „	viz. grain 11 „	82·50	17·50
		straw 52 „	79·50	20·50

The difference between the amount of produce, in the bed left unmanured, and in the rest, sufficiently attests the benefit that had been derived from every one of the saline matters added, and that nearly in an equal ratio from each of them; whilst the larger amount of soda present in the crops which had been dressed with carbonate of soda and with chloride of sodium, might seem to indicate, that this alkaline principle had, to a certain extent, taken the place of the potash, in consequence of its being present in the soil in a larger proportion here than in the other plots.

But a little further consideration may perhaps suggest a different mode of explaining the facts in question.

Let us suppose that there may be circulating through the system of a plant, during all the stages of its growth, a certain amount of such saline matters as happen to lie in contact with its roots, of which, although only that which is capable of being assimilated by its organs actually constitutes a part of its substance, the remainder is nevertheless detained within the vegetable tissue for a certain time, and cannot be separated from it by any known mechanical process[h].

When therefore we dry a plant, and reduce it to ashes, its residuum after incineration will contain, not only the alkaline matter which actually entered into the composition of the organs themselves, but likewise that circulating at the time through its tissue. The former, indeed, may be assumed to bear a constant relation to the volatilizable ingredients associated with it, but the latter will necessarily vary according to the nature of the saline impregnation belonging to the water which bathes its roots.

Thus, from the soil to which nothing had been added, a certain amount of soda as well as of potash would seem to have been extracted by the barley grown in it, since both alkalies made their appearance in the portions incinerated. This, however, is easily explained, inasmuch as, by a previous analysis, both soda and potash had been found to exist as original constituents of the soil. It is probable, however, that a good deal of the estimated quantity of each did not actually enter into the constitution of the organs, but had been merely arrested in its progress through the plant, so as to be present in it at the moment when the crop was cut down.

From the addition of carbonate of potash to the soil, only a small increase to the percentage of potash in the plant itself appears to have resulted; but it must be recollected,

[h] This, of course, may be assumed to be the case with regard to the *normal* ingredients of a plant, even though it be admitted that my subsequent experiments, detailed in the next memoir, have proved, that it does not hold good with respect to *abnormal* ones. (May, 1867.)

that the quantity of potash present in the ground was distributed over a greater amount of crop, as the produce of the beds rose, in consequence of this addition, from 63 to 71½ lbs.

By the dressing of carbonate of soda given to the third set of beds, the per-centage of soda was augmented in a still higher ratio, because the increase of crop which followed did not in the same degree augment the consumption of soda by the plant, as this alkali enters less abundantly into its actual organization. A larger proportion of it, therefore, would remain in the juices circulating through the vegetable tissue, and would therefore add to the apparent amount of soda in the crop when examined.

The same explanation may apply to the increase of the same alkali which took place in the crop that had been manured with common salt; this, however, would appear to exist in the plant as common salt, since, judging from the results of the analyses given in my Bakerian Lecture[1], the salt in question enters, as such, into the tissue of a plant without undergoing decomposition.

I may perhaps elucidate my meaning by the following hypothetical statement, in which the numbers given must be understood to be quite arbitrary, and intended merely for the purpose of illustration.

Confining ourselves to the grain of the barley experimented upon, let us assume the normal quantity of potash and soda present in it to stand to each other in the relation of 74 of the former to 9 of the latter.

Then, in the unmanured crop:

Normal quantity being .	Potash	74·0	Soda	9·0	
There will have been circulating through the crop when cut .	„	8·5	„	8·5	or 50 to 50.
		82·5		17·5	

In crops manured with carbonate of potash :

[1] Phil. Trans. for 1845.

Normal quantity . . .	Potash	74·0	Soda	9·0	
Circulating through the crop	„	10·5	„	6·5	or 67 to 33.
		84·5		15·5	

In the crops manured with carbonate of soda and chloride of sodium :

Normal quantity . . .	Potash	74·0	Soda	9·0	
Circulating through the crop	„	2·5	„	14·5	or 15 to 85.
		76·5		23·5	

The above explanation may perhaps be open to objection ; but I conceive it to be at least encumbered with fewer difficulties than would attend the notion of an actual substitution of soda for potash taking place within the organism of a plant ; for in that case we must suppose, that although a certain number of the atoms of the one alkali may be replaced by the other, no such change can take place with regard to the greater part of those present ; for were the latter possible, that is, were the whole, or the greater part of the atoms of potash capable of being replaced by soda, not only would there have been a much greater difference in the constitution of barley grown in soils so largely impregnated, as mine were, with salts of soda, in comparison with the same charged with those of potash ; but we should also be at a loss to explain the absence of a larger proportion of soda-salts in those samples of barley which I had obtained from the neighbourhood of the sea, than existed in others taken from inland situations.

In the experiments above detailed, the utmost increase of soda in the crop, caused by so liberal an application of soda salts to the soil, did not exceed 8 per cent. of the whole ; or in other words, if we adopt the hypothesis of substitution, only 1 atom in 12 of the potash actually present admitted of being replaced by soda : nor does it appear, that the largest disproportion between the quantity of the two alka-

lies brought into contact with the plant can materially affect this limitation; for otherwise, marine algæ, which are nourished by a fluid in which the potash stands in the smallest possible relation to the soda, could scarcely contain so large an amount of potash as is frequently found to exist in them. And yet, although we must, in adopting this hypothesis, assume certain limits to exist, beyond which the usual constitution of a plant does not admit of change even by the most liberal supply of soda, a large excess of this latter alkali must be supposed necessary, in order that it should replace the potash in any degree at all; inasmuch as we find no extraordinary amount of the mineral alkali in barley grown near the sea, where soda-salts must be more abundant than they are inland.

These considerations would lead me to imagine, that even in marine plants, a portion of that large quantity of soda, which is detected in their ashes, may be derived from the juices circulating through their cellular structure, and does not enter into their actual organization; so that even in them, perhaps, potash may turn out to be the alkali which plays the principal part in building up the living fabric of the plant. I am by no means inclined, however, to limit the uses of the alkaline matter which is constantly contained in the sap, to the one end of supplying a plant with the potash and soda which it requires for the formation of its different organs, and for that of its characteristic secretions; since Liebig himself has pointed out another function which both alkalies are equally fitted to discharge, namely, the fixation of carbonic acid, through which, owing to the consequent diminution of oxygen, sundry other organic acids, such as the tartaric, the oxalic, the malic, &c., may be formed.

It is not my intention, however, to pursue this fertile subject on the present occasion; in conclusion, therefore, I will only remark, that I am fully aware of the scantiness of the data contributed by myself in this paper towards the elucidation of the question discussed in it; and that I should have been reluctant to occupy the time of this Society by bringing them forward, had I not hoped that they might be the

means of stimulating others of our members, who have more leisure than myself for such investigations, to undertake a larger and more precise train of experiments, calculated to determine the still questionable point, as to the power residing in plants to substitute one mineral ingredient for another, in the construction of their respective organs, or in the elaboration of the peculiar secretions which they contain.

On the Power ascribed to the Roots of Plants of Rejecting Poisonous or Abnormal Substances presented to them.

(*From the Quarterly Journal of the Chemical Society, vol. xiv., 1862.*)

BEFORE reporting to the Society the results of a few experiments which I have lately made on the subject of the absorption of mineral bodies by the roots of plants, it may not be amiss for me to enter into some general account of the state of our knowledge relative to the subject referred to, including a brief notice of the experiments tending to elucidate it, which have been already communicated to the public by myself and others.

I may in the first place remind you, that, with regard to the functions of the roots, two opposite views have been entertained: the one, that they consist simply of a system of cells, surrounded by membrane, which possesses no peculiar properties, but is capable, by virtue of endosmose, of absorbing any fluid that happens to lie in contact with their external surfaces, and, by the force of capillarity, of transmitting the same afterwards to the parts above: the other, that as vital organs they are endowed with a certain power, which seems to us analogous to selection, or, at least, answers the same purpose; in consequence of which they abstract from the fluid presented to them those principles which are required for the general uses of their own economy, and that of the plant in general.

The former, doubtless, is the more readily intelligible hypothesis, and is one in support of which many facts may be alleged.

Saussure, for instance, found, that when the roots of a water-plant, such as the *Polygonum Persicaria*, were im-

mersed in water containing equal quantities of various saline and other soluble bodies, the latter were absorbed, not in the ratio of their respective fitness for supplying the wants of the system, but in proportion to the facility with which the solution was capable of entering the spongioles, and of flowing through the tissues of the plant.

Gum and humus, for instance, being viscid, entered the spongioles in much smaller quantity[a] than various saline bodies did, which might be regarded as less needed by the plant which absorbed them.

But, of all those tried, no one was taken up in such large quantities as sulphate of copper; a circumstance which may be attributed to the poisonous and corrosive quality of this substance, owing to which the texture of the cells became disorganized, and the entrance of the solution into the vegetable texture took place as freely, perhaps, as if the parts had been actually severed asunder.

These statements of Saussure were confirmed by Sir Humphry Davy, who, in his "Agricultural Chemistry," pointed out, that saline solutions are absorbed more readily when in a diluted state than when concentrated.

Later experimentalists have even gone so far as to contend, that, with the exception of such salts as those of copper, which destroy the texture of the roots, all solutions are absorbed with equal readiness. Such was the conclusion arrived at by Dr. Lindley, from researches of his own, which are detailed in vol. xviii. of the "Annals of Natural History."

The experiments of Boucherie have also been appealed to, as affording an argument from analogy in support of this same view. In these, openings were made in the lower part of the trunks of various trees by sawing out a portion of the wood, and a waterproof bag was placed round the severed portion, so that a saline solution might be introduced, which was rapidly absorbed by the tree, of whatever

[a] The late researches of the Master of the Mint on endosmose may serve to explain this, on the principle that gelatinous bodies do not pass through membrane.

nature it might be[b]. Through this device, the wood became impregnated with various metallic salts, by which its hardness and durability were augmented in a remarkable degree.

Similar experiments have been instituted by Mr. Hyett, of Painswick House, near Gloucester, the results of which are given in vol. viii. of the "Transactions of the Highland Society," published in 1843.

The same inference has been deduced from observations made as to the influence exerted upon the constitution of a plant by the quality of the soil, or by the nature of the ingredients present in it which are submitted to its roots.

This influence was shewn by the experiments of Saussure, where a fir, which had grown at Mont Breven on a dolomitic rock, was proved, when burnt, to contain, in its ashes, a certain per-centage of magnesia; whilst the same species from Mont la Salle, a granitic rock, was destitute of magnesia, but possessed an equivalent increase in the lime and potassa belonging to it; and likewise from those of Berthier, shewing, that the ashes of a fir which had grown in Norway, yielded only 16·8 per cent. of alkalies, 29·6 of lime, and 3·3 of magnesia, whereas the same kind of timber from Allevard, in France, afforded 34·7 of alkali, 13·6 of lime, and 4·35 of magnesia.

Such are the principal facts that may be appealed to in support of the hypothesis, that roots are merely passive instruments, which take up whatever is presented to them without selection or preference.

It must, however, on the other hand, be observed, that the properties of endosmose, capillarity, and the like, to which the absorption of saline fluids by the roots, and their transmission through the stem, may be attributed, are common to all tissues similarly constituted, whether dead or living; and that the co-existence of other properties more especially vital, is by no means excluded by their presence.

It is just the same, indeed, with the animal kingdom. No one doubts, that the living organism of animals is amenable to the common laws of matter, however much

[b] *Annales de Chemie*, vol. lxxiv. p. 113.

the latter may be controlled and masked, as it were, by the forces of life.

That in the case of the spongioles also, similar functions dependent upon vitality must be assumed to be superadded to those common to matter generally, may be argued from the fact, that the power of substituting one ingredient for another is at any rate circumscribed within very narrow limits.

The most trustworthy experiments hitherto made, appear to shew: 1st. That it is impossible to replace any of the bases or acids which usually enter into the composition of the vegetable by any other of the same series which are not usually present: 2ndly, That even those bases which are capable, to a certain extent, of replacing each other, cannot do so indefinitely: and, 3rdly, That much of the difference in composition which has been remarked as existing between plants of the same species, growing in different soils, or manured in various ways, may be accounted for by the predominance of one or other of its proximate principles in the several samples submitted to examination.

Thus wheat cultivated in one soil, may contain more gluten and less starch than in another, and hence the chemical constitution of the grain must in each instance necessarily vary.

In support of these positions, I will appeal to some experiments of my own already published[c], as well as to those of others; and I will also allude to certain facts of general notoriety which seem to me to point in the same direction.

In the Transactions of the Linnæan Society, vol. xvii., published in 1835, I shewed, that plants watered with a solution of nitrate of strontia, and seeds sown in soils containing the sulphate of the same earth, never appeared to absorb any portion of this base: for it could not be detected in their stems, leaves, or flowers, and only in the minutest quantity in, or attached to, their roots.

In relating these experiments, I took the opportunity of alluding to an analogous result obtained by myself with reference to the animal kingdom, where hens, allowed access,

[c] See the last Memoir.

during the breeding season, to sulphate of strontia, but to no other earth, laid eggs with soft shells.

I may also appeal to the notorious fact, that no acid can replace the phosphoric in the vegetable organism; a principle upon which the entire theory of scientific agriculture, as expounded by Liebig and others, seems to be based. Were this not the case, the necessity of supplying, from time to time, the mineral phosphates, as fast as they are drawn off by successive crops, might be superseded by other expedients. Some interesting experiments, bearing upon this subject, are reported on the authority of the Prince of Salm Horstmar, in the *Annales de Chimie*, for 1850, vol. xxxiii. The Prince moulded some little wax vessels, in height about $2\frac{1}{4}$ inches, and in diameter 1 inch, and filled each of them either with pure sand, or with powdered rock-crystal which had been previously calcined. Into these vessels were introduced a few grains of oats, and along with them a minute quantity of one or more of the ingredients which, by analysis, this plant has been found to contain.

The following were the results arrived at :—

1. When neither any inorganic nor nitrogenized materials had been added, the oats sprouted, but came up weak, and were reduced to two flowers and a single fruit.

2. When nitrogenized substances were added, the plant shot up higher, but was too feeble to sustain itself in an erect position.

3. When denied access to nitrogenized materials, but supplied with all the inorganic principles normally present in it, namely, with silica, potassa, lime, magnesia, and oxide of iron, together with phosphoric and sulphuric acids, the plant was small and languid, and whilst the formation of flowers took place to a more limited extent than in the former case, that of grain was altogether wanting.

4. When both the mineral substances above enumerated and the nitrogenized matters were present, there was an increase both in the vigour of the plant and in the number of its flowers; but still something appeared to be wanting in the pulverized quartz, which existed in the river sand; for, although lateral shoots were produced, shewing a power

of development to reside in the plant, still the regular formation of grain did not take place.

5. But when any one of the above seven inorganic substances was wanting, its development became in some way or other arrested.

Thus when magnesia was not supplied, the growth of the plant was feeble; when potassa, it was stunted, and only a single stem produced; when soluble silica, the growth of the stem was arrested; when phosphoric acid, only one flower was obtained, and this did not produce grain; when sulphuric acid was not supplied, there was no stem, and the plant died before the third leaf had been produced; and when iron was wanting, the plant was deficient in its natural green colour.

The experimental results above obtained are decidedly adverse to the idea, that even normal substances can replace each other indefinitely in the structure of a plant, shewing, as they seem to do, that the constituents are all of them indispensable to their healthy growth.

The fourth conclusion indeed arrived at, namely, as to the imperfect development of a plant grown in powdered quartz, even where all the bodies known to be essential to it are provided; although it arrives at a perfect state of maturity, in all respects, when grown in river sand, seems to indicate, that we are still unacquainted with all the ingredients of the soil and of the plants grown in it.

This is the point, therefore, towards which our inquiries should be directed, and which the Prince himself, at the end of his Memoir, proposes to investigate. As, however, ten years have since elapsed without any further communication from him having reached my knowledge, I conclude that we must not expect any further elucidation of the subject from this quarter [d].

[d] Since the reading of this Paper, I have found, from a late notice in the Chemical News, copied from Poggendorff's *Annalen*, that the Prince now believes lithia to be indispensable to the development of grain, he having lately found, that barley did not come to maturity in coarsely powdered and washed rock-crystal, even when supplied with carbonate of lime, magnesia, and manganese, sulphates, phosphates, and fluoride of potassium; but that the addition of

In the interval, however, the remarkable researches of Bunsen and Kirchhoff, on Spectrum Analysis, have established the existence in nature of some substances hitherto unsuspected, and the wide diffusion of others hitherto regarded as of great rarity; so that, as I perceive has been already suggested by Dr. Cameron in a paper read before the Dublin Society [c], the failure of our crops may, in certain cases, be owing to the absence from the soil of some ingredient hitherto overlooked, such for instance as lithium, or the new alkali-metal cæsium, which had been withdrawn by preceding crops, and not restored by the species of manure which had been subsequently applied.

If then, it be true, that abnormal substances cannot supply the place of normal ones, and, therefore, do not enter into the organization of a plant, we have only the alternative of supposing, that when brought into contact with the roots, the former either are not admitted at all into the system, or are afterwards excreted from it.

Of the two, the former would seem to be the more legitimate inference; for if these bodies had been excreted, we ought to find traces of them *in transitu* distributed throughout the various tissues of the plant.

The latter is, indeed, actually found to be the case, when substances of this nature are brought into contact with the cut surfaces of the stem or roots, as in the experiments of Boucherie; or even when the vitality of the root is destroyed by the application to it of a poisonous or corrosive substance, as in the cases reported by Saussure.

On this principle, indeed, it is easy to colour the stem of a plant blue, by first immersing its roots in a solution of sulphate of iron, and afterwards in one of ferrocyanide of potassium, when the ferrocyanide of iron will be found wherever the solutions intermix. But no such transmission upwards would seem to take place when an abnormal substance is brought into juxtaposition with a living root, until its or-

nitrate of lithia, in the proportion of the 100th of a milligramme, caused it to flower, and to yield perfectly ripe grain.

[c] See the "Gardeners' Chronicle" for March 29, 1861.

ganization is destroyed either by mechanical means, or by the poisonous influence of the application itself.

The case, however, is different, when we employ substances normally present in the plant. Here there can be no doubt, that the substances do obtain admission into the system, and that the usual proportions of the respective ingredients may be made to vary, by supplying the roots more plentifully with one ingredient than with the rest.

It has been stated, for instance, by Will and Fresenius, that wheat cultivated near the sea contains more soda and less potassa than it does in more inland situations; and although certain analyses made in my laboratory by Mr. Way, some years ago, would lead to the contrary conclusion, yet in a Memoir read before the Chemical Society in 1853, I brought forward facts which lend support to the view taken by the German chemists, by shewing that the relative proportions of potassa and soda present in barley varied, according as the soil was more or less impregnated with one or other of these alkalies [f].

It will be seen, by reference to the report of these experiments contained in the Chemical Journal, that in this manner the proportion of soda to potassa was made to vary from 23·5 to 15·5 in the grain, and from 27·25 to 20·0 in the straw, thus appearing to shew that, to a certain extent, one alkali might replace the other in the vegetable organization.

But the latest and most extensive series of investigations bearing upon this subject are those by Malaguti and Durocher, recorded in the *Annales de Chimie* for December, 1858.

The primary object of these chemists was to determine, whether any relation subsisted between the mineral constitution of a plant and its organic structure, as evidenced by the place it occupied as a member of one of the natural families into which the vegetable kingdom is divided.

For this purpose they examined the ashes of 150 plants, selected from twenty-five different natural families, and growing spontaneously in the soil of France, analysing at least three species of each family, in order to estimate the

[f] See the last Memoir.

range of variation occurring in them, and distinguishing the arborescent from the herbaceous, as containing a different amount of ligneous fibre.

In the course of these inquiries, two facts were elicited, both of which accord with the results obtained by myself from the experiments above referred to; the first, that plants must possess a power of selection, inasmuch as several species growing on the same soil, abstract from it different amounts of the normal constituents presented to them; the second, that the same species, raised upon different soils, vary to a certain extent in their mineral constitution.

Thus, in argillaceous soils, the proportion of chlorine in the plant is, in general, less than it is in calcareous ones, the range of variation being from 4 to 20 per cent. On the other hand, the amount of sulphuric acid will be greater in argillaceous than in calcareous soils; and the same remark applies, although in a less degree, to the phosphoric acid also.

In like manner, plants grown on calcareous soils contain, in general, more soda and less potassa than in argillaceous ones. Thus, in *Brassica napus* there was present, of soda 5·50, and of potassa 12·34, when grown on a limestone; and of soda 3·00, potassa 25·00 when cultivated upon clay.

Similar differences are pointed out as existing in the case of *Trifolium pratense* and *T. incarnatum*, &c., shewing the dependence of the alkaline ingredients upon the chemical constitution of the soil in which these plants were reared.

Liebig, in his "Agricultural Chemistry," has pointed out, that in these cases of substitution, the absolute amount of bases present must be exactly equivalent to the acids which the plant contains, or, in other words, that the sum of the oxygen in them is invariable.

Thus, according to Saussure's analysis already referred to, 100 parts of the ashes of a fir from Mont Breven contained—

Carbonate of potassa .	3·60	in which the O. was	0·415
„ Lime .	46·34	„	7·327
„ Magnesia	6·77	„	1·215
		Total of O. .	9·007

whilst trees of the same species from Mont la Salle contained :—

Carbonate of Potassa	.	7·36	in which the O. was	0·850
,, Lime .	.	61·19	,,	8·100
,, Magnesia		0·00	,,	——
			Total of O.	8·950
			Difference only . .	0·057
				9·007

And in like manner the ashes of a fir from Allevard, according to Berthier, contained

Of potassa and soda together	16·6	containing of O.	=	3·57
Lime	29·6	,,	,,	8·36
Magnesia	3·3	,,	,,	1·26
				13·10

whilst those of one from Norway contained

Of potassa . .	14·10	containing of O. =	2·40
Soda	20·70	,,	. 5·30
Lime . . .	13·60	,,	. 3·82
Magnesia . .	4·35	,,	. 1·69
			13·21
		Difference .	11
			13·10

These remarkable approximations equally attest the accuracy of the analysts, and the acuteness of the reasoner who first pointed out the relations between the two ;—they prove, that the amount of bases is determined by the quantity of acids contained in certain tissues of the plant, and that this amount, however much the nature of the acid may vary, is in each case definite.

But although the quantity of bases present is necessarily dependent upon the amount of acid contained within the plant, it does not follow that even isomorphous bodies, such as potassa and soda, and still less other bases, as lime and

magnesia, are capable of replacing each other throughout the entire organism.

The contrary, indeed, would seem, from the following considerations, to be the more probable hypothesis.

In the experiments performed by myself, which have been already noticed, it may be remarked, that the difference in the relative proportions of soda and potassa never rose above 8 per cent. ; and yet, if this proportion of soda had taken the place of potassa as a constituent in the organism of a plant, one does not see why the same might not be the case with respect to the whole. On this supposition, the organs of plants would, like the different varieties of augite, garnet, &c., have no definite or fixed composition, the only condition being that their elements should be isomorphous : a position well established with regard to the mineral kingdom, but scarcely applicable, one would think, to the vegetable.

Perhaps the strongest proof that can be adduced in favour of the necessity of a certain fixed chemical constitution for the existence of plants is supplied by the case of those which derive their nourishment from sea-water.

If we examine chemically any of the marine algæ, we shall find, that, although containing a considerable proportion of soda, yet, that in many of them, there is present an almost equal amount of potassa. Thus, in the 4th edition of Liebig's "Chemistry of Agriculture," 1847, is given the analysis of four species of *fucus,* two of *laminaria,* and twelve of other kinds of algæ. Four of these analyses were made in the laboratory of Giessen; and for the remainder we have the authority of Forchhammer ; they present, indeed, many discrepancies, but, upon the whole, concur in establishing, that potassa as often exceeds soda in the constitution of an alga, as the reverse.

I should, indeed, from the difference in the results obtained, be tempted to infer, that a large but variable quantity of sea-water had been circulating through the tissues of the sea-weeds at the times when they were severally collected, and that this had been included in the respective analyses. At any rate, if soda were capable of replacing potassa in the

system of a plant, one does not see why any potassa at all, except an infinitesimal quantity, should be present in them, considering that sea-water, which alone can supply the mineral constituents present in the organization of a marine plant, contains an infinitely smaller amount of potassa than it does of soda.

The case might have been put even more strongly, if I had adverted to the quantity of iodine which sea plants extract from the waters of the ocean, although the proportion of that ingredient, which is known to exist in the latter, can, from its minuteness, be hardly detected, except by the most delicate tests.

Hence iodine, as it would seem, can no more be replaced by chlorine in the organism of marine plants, than potassa can by soda, although both the one and the other are regarded as isomorphous.

Liebig, in his "Letters on Modern Agriculture" (page 44), has illustrated this principle by the case of the common duckweed of our ponds, the *Lemna trisulca*, growing in a bog, the water of which contained the following salts, in such proportions, compared with those present in the plant, as indicated a power of selection residing in the latter.

Thus there were present in seventy-six parts :—

	of the soil	of the duckweed.
Lime	35·000	16·820
Magnesia	12·264	5·080
Chloride of sodium . . .	10·100	5·897
Potassa	3·970	13·160
Soda	0·471	. .
Oxide of iron and traces of alumina	0·721	7·360
Phosphoric acid	2·619	8·730
Sulphuric acid	8·271	6·090
Silica	3·240	12·250
Chloride of potassium . . .	. .	1·450
Total . . .	76·656	76·837

We must here admit a certain power of selection residing in the roots of the plant, since, whilst the water contained

in 76 parts, of lime about 35, and of magnesia 12 per cent., the plant had abstracted only 16·8 of the former, and 5 of the latter element; and whereas the proportion of potassa in the water was less than 4, that in the plant was 13; of phosphoric acid in the water 2·6, in the plant 8·7; of silica in the water 3·2, in the plant 12·2.

But, in fact, we need not go so far for proofs of this familiar principle; for, as every plant possesses a different chemical constitution, it is evident that of several species grown in the same soil, each one must exercise a kind of selection, in order to extract from the same material the several ingredients which it requires for its separate organization.

I have already[g] attempted to reconcile the apparent discrepancy between the above two classes of phenomena,—namely, the variations in the composition of a plant which may be induced by artificial means, and the necessity for the presence of certain specific mineral ingredients in their organization, by supposing that the proximate principles existing in a vegetable are fixed and invariable, a seed possessing a certain per-centage of phosphoric acid which cannot be replaced by any other ingredient, a mass of ligneous fibre, a certain amount of potassa, &c.; but that, independently of the acids and bases which enter into their composition, and make up the fabric of the plant, there is always circulating through its tissues a certain amount of alkali, one function of which, according to Liebig, is to promote the fixation of carbonic acid; and that, as the latter duty may be discharged by one alkali as well as by another, it is conceivable, that potassa and soda, or even lime and magnesia, may replace one another, so far as respects the fluids which are circulating through the vegetable tissues.

Hence we may understand how it is, that one portion of the alkaline or other mineral ingredients, present in a plant, may be definite as to quality, whilst another portion may vary according to circumstances; and accordingly, whilst

[g] See the last Memoir.

it is found that each proximate principle presents a fixed chemical composition, the entire plant may, nevertheless, vary within certain limits, as it will contain an uncertain amount of unassimilated sap flowing through its system at the time when it was cut down.

There is also another cause for the different composition of the ashes of the same species of plant, namely, a variation in the ratio subsisting between the proximate principles themselves. Thus, one particular climate, or mode of culture, may favour the production of gluten, another that of starch; and thus the seed of the same plant, in different localities, may not always be identical in point of composition. Thus, I have given instances, in my "Lectures on Agriculture," published in 1841, in which the application of nitrate of soda to a soil added 4·25 to the amount of gluten present in the crop of wheat growing in it; and Davy affirms that the flour of warm climates contains, in general, more gluten than that of colder ones [h].

Upon the whole, then, I should be inclined to infer, that the spongioles of the roots have residing in them some specific power of excluding those constituents of the soil which are abnormal, and, therefore, unsuitable to the plant, but that they take up those which are normal in any proportions in which they may chance to present themselves; the redundant portion, however, left after the necessities of the organism have been provided for, being again excreted, so that a vegetable will only vary in mineral composition in proportion to the relative amount of the several proximate principles which may be produced under the peculiar circumstances of its cultivation, or of the climate it enjoys.

If it be questioned, whether we can concede to the absorbing surfaces of the roots a function so nearly allied, in semblance at least, to volition, I would refer to the observations of Huxtable and Thomson, and more especially to the valuable Memoirs of Way, as establishing the existence of a property connected with these organs of no less extraordinary a nature; namely, that whilst the carbonate of

[h] See also Boussingault, *Ann. Ch. Phys.* [2], lxv. 301.

ammonia, present in rain-water and in manures, is so firmly fixed in the soil, that, under ordinary circumstances, water containing carbonic acid seems unable to withdraw it, the same is nevertheless commonly absorbed and conveyed into the tissue of plants by means of their roots.

Hence, as Liebig observes, vegetables must obtain their nourishment by virtue of some specific attraction exerted by the rootlets upon the ingredients locked up within the earth; a property, wonderful, indeed, but not more so than that possessed by the leaves, of converting carbonic acid into sundry organic principles by the elimination of oxygen.

In a Paper published in the Philosophical Transactions for 1836, "On the Action of Light upon Plants, and of Plants upon the Atmosphere," I concluded, that light operates upon the vegetable, as it does upon the animal kingdom, in the character of a specific stimulus, so that the formation of the various secretions elaborated from the carbonic acid absorbed, is not a purely chemical process, but is due to some peculiar agency of its green parts, connected with, and dependent upon, their vitality.

I would now extend the same inference to the roots, considering that, when in a healthy and vigorous condition, they serve as instruments, for conveying to the other parts of a plant those juices which contain substances calculated for affording them nourishment, and for excluding others which might be useless or injurious to them.

Liebig, indeed, goes so far as to contend, that the mineral matters presented to the spongioles are not in a soluble form; for if they were, he suggests, that water, either alone or in conjunction with carbonic acid, would be capable of abstracting them from the earth. He therefore imagines, that, being distributed over the general body of the soil in a finely divided condition, they are taken up by the rootlets, owing to a kind of specific attraction. We can thus more readily understand how it is that certain ingredients are absorbed in larger quantities than others, and why some even should be absolutely rejected, as in the case of the common duckweed, to which Liebig refers.

It is remarkable that, although alumina is not usually

present in the ashes of plants, yet that one tribe, namely, the *Lycopodiaceæ*, contains it in large quantities [i],—a fact which I have myself verified.

By what precise agency this usually insoluble earth [j] finds its way into the vegetable economy seems to be yet undetermined; but its occurrence in a few plants may be regarded as a proof that it would have been taken up in all, if the spongioles were merely passive instruments for absorbing and transmitting indiscriminately whatever was presented to their external surfaces.

But, indeed, the whole efficacy of geothermal culture, now so much insisted on by horticulturists, seems to presuppose that the roots are vital agents, capable of being acted upon by stimuli. Why else should the application of heat to the roots impart greater vigour to the upper parts of a tree, and cause, as in the case of the *Bougainvillea*, the copious development of flowers where none before were produced; and, on the other hand, why, as happened in so many cases during the late severe winter [k], should the plant be destroyed in consequence of the cold penetrating into the ground, whilst it resisted the influence of the frost so long as its roots were protected?

If the spongioles acted upon the surrounding matter simply by virtue of endosmose, as a dead membrane might do, the process of absorption, although it might be arrested by frost, would be renewed when the mild weather returned, and the plant would sustain no further damage than what might be due to a temporary suspension of its functions; whereas the consequence of the late extreme cold appears to have been, that the roots became unfitted for discharging their office, and that the stem and branches suffered, not only negatively, by the deprivation of proper nourishment, but positively, by receiving noxious matter from the ground. In short, the spongioles may be supposed to have

[i] *L. chamæcyparissus*, 39·07; *L. clavatum*, 20·69; according to Ratthausen, in Liebig's *Jahresbericht*, for 1853, p. 586.

[j] The late researches of the Master of the Mint, which were not known to me at the time of the reading of this Paper, shew that alumina is capable of existing in a soluble condition combined with water.

[k] I allude to the winter of 1860-61.

been reduced to the same condition by the cold, as that to which they would be brought by the application of a poisonous solution, such as the sulphate of copper already alluded to; and in both cases the same consequences would ensue.

Let us now consider, how the influence of poisons upon the vegetable economy is to be reconciled with the principles just laid down.

It is notorious, that a moderate dose of certain mineral as well as of vegetable poisons applied to the roots of a growing plant destroys its vitality, as certainly and as speedily, as if the same were injected into any part of the stem or branches. It is, therefore, evident, that the poison does not merely exert a *local* influence upon the part to which it is applied, but that it must be absorbed and transmitted through the other parts of the organism. But how, it may be asked, is this fact to be reconciled with the power of rejecting such abnormal ingredients as are presented, which I have ascribed to the roots[1]? The reason I conceive to be, that the deleterious quality of the substance destroys the vitality of the part, and thus reduces it to the condition of a simple membrane, which by endosmose absorbs whatever is presented to its external surfaces.

The same thing may be supposed to happen generally in the case of poisons of a certain degree of intensity, as in that of the solution of sulphate of copper, which Saussure and others have fully ascertained to be taken up in larger quantity than any other soluble material. In all such instances, however, the absorption of the poison was speedily followed by the death of the plant.

When, on the contrary, the poison is presented in a more diluted form, the effect resulting would seem to be a matter of rather greater uncertainty, as the statements made on the

[1] One of the few cases recorded, on competent authority, in which an abnormal substance appears to be taken up by a plant, without, apparently, affecting its healthy condition, is that of the violet which grows on the calamine rocks near Aix-la-Chapelle, and is found to contain zinc amongst its mineral constituents. See "Annals of Natural History," vol. xiv., second series, for 1854.

subject are of a conflicting nature. On the one hand, Dr. Edmund Davy, Professor of Agriculture and Agricultural Chemistry in the Dublin Society, is quoted in the "Gardeners' Chronicle" for September 10th, 1859, as having ascertained, that peas watered with a saturated solution of arsenious acid for more than a week continued to the last uninjured, although arsenic was readily detected by appropriate tests in the plants subjected to this treatment. He is also reported to have stated, that a solution of superphosphate of lime, which, if impure sulphuric acid has been employed in its preparation, always contains more or less arsenic, was applied to a cabbage plant, which nevertheless grew and flourished, although arsenic was detected in it afterwards. A similar experiment was tried also upon turnips, with the same results. Mr. Horsley, of Cheltenham, has since published a letter addressed to Dr. Edmund Davy, in which he confidently affirms the same fact, and assures us that it holds good in the case of all the various plants upon which he had experimented.

On the other hand, Jäger, Humboldt, and Link could not induce plants to grow when sprinkled with arsenicated water, and Mr. Ogsden (in the "Gardeners' Chronicle" for March 10th, 1860,) states, that on repeating Davy's experiments, he found cabbage plants, and also Scotch kale, to droop and die within a week after the application of the arsenical solution. In neither of these cases was any arsenic detected in the leaves, or higher up in the stem, than about 5 inches from the ground.

In corroboration of these results, it might be unfair to add those arrived at by Marcet, as he plunged the shoot of a kidney-bean, and not its roots, into the arsenical solution made use of, which contained, it seems, 2 grains of arsenious acid in 2 ounces of water. Under such circumstances, the death of the plant which ensued cannot well be cited as an argument from analogy as to what might happen in the case of the spongioles of the roots; but the immunity which the potatoe crop in the neighbourhood of Swansea seems to enjoy from the influence of the arsenical fumes with which the atmosphere thereabouts is so infected, and likewise the

fact, that, in certain parts of Cornwall, where arsenical pyrites from the mines is thrown upon the soil in large quantities, the crops do not appear to suffer, and contract, in consequence, no injurious properties, may perhaps be fairly adduced in support of this same conclusion.

I will now lay before the Society a few experiments bearing upon the same subject which have been carried on by myself.

I possess, in the first place, some notes of an experiment made so long ago as 1837, in which plants were watered with a solution of arsenious acid in the proportion of 1 grain of the mineral to 3 pints of water, and where no trace of arsenic could be detected in them after the application.

Passing over these trials, which appear wanting in the requisite precision, I will state what I have done during the past year, with a view of determining the power of the roots to absorb poisons and abnormal substances, when presented to them in a state of great dilution.

Having first made a few tentative experiments on a small scale, with the view of ascertaining what amount of arsenious acid might be applied to a growing plant with impunity, I watered a plot of ground, 25 feet long by 4 feet wide, containing a crop of young barley, with a solution of arsenious acid, in the proportion of 2 ounces to 10 gallons of water. Finding, six days afterwards, that the barley appeared to be blighted, I applied to a second plot, of the same dimensions, a solution of just half the strength of the preceding, containing 1 ounce of arsenious acid to 10 gallons of water.

After two applications, with an interval of twelve days between, viz. on July 16th and 28th, this crop also appeared to be somewhat damaged; but nevertheless the application was renewed on August 11th, 18th, and 21st, so that, on the whole, the solution was applied five times without preventing the crop from arriving at maturity.

A similar plot of ground sown with turnips, was watered on the 3rd, 10th, 17th, and 27th of September, with a solution containing 1 ounce of acid to 10 gallons of water; so

that, on the whole, 4 ounces of arsenious acid diluted with 40 gallons of water had been added.

Upon the turnips no sensible effect was produced by the application, the bulbs appearing as large, and the tops as luxuriant as they were in the rest of the bed which had not been so treated.

It may be worth remarking, although perhaps the circumstance might be owing to other causes, that the barley so watered arrived at maturity a fortnight earlier than the rest of the crop.

The produce was somewhat less than the average in the other parts of the field, which in every 100 square feet was found to yield 12lbs. of straw, and 4lbs. 14 oz. of grain; the produce in the portion treated with arsenic being $10\frac{1}{2}$ lbs. of straw, and $3\frac{1}{2}$ lbs. of grain. The weight of the latter to the bushel was 52 lbs., whereas, that of the other portion was only 50 lbs.

The turnips from the arsenicated portion when arrived at maturity, yielded of

Roots . . .	51 lbs.
Tops . . .	20 „

whilst a similar portion of the same bed not treated with arsenic, yielded

Of roots . . .	58 lbs.
Tops . . .	22 „

Thus much with respect to the influence of the poison upon the amount and quality of the production.

With the view of ascertaining whether these crops had really taken into their system any appreciable quantity of arsenic, the following method was adopted.

1st. With respect to the barley.

A portion of the grain was burnt in an earthen crucible, with plenty of nitrate of potassa, and the ashes were treated with hydrochloric acid. Sulphuretted hydrogen was then passed through the solution, but no precipitate was occasioned by its addition.

As, however, it might be suspected that the arsenic had been driven off by the heat, notwithstanding the presence of nitre, by which it was intended to fix it, a second portion of the grain was heated in a glass retort for 5 hours together, with some nitro-hydrochloric acid. The liquor was maintained at a boiling temperature, and the vapour that came over collected and tested with sulphuretted hydrogen as before.

In this instance, a slight precipitate was obtained, which afforded indications of arsenic by Marsh's test.

As, however, it was possible that a trace of this substance might have adhered to the external surface of the husk, from its having been sprinkled with the solution, a second portion of the barley was carefully washed before subjecting it to the same treatment, and here the indications were much less decisive, a very slight stain being perceived upon the subliming tube, but one of a reddish colour, and less distinctly arsenical in its appearance than the former.

The suspicion thus created, as to the presence of arsenic in the case of the barley being merely superficial, was confirmed by my examination of the turnips, which had been similarly treated with an arsenical solution; for here the root, after being sliced into thin pieces and dried, was treated with a solution of chlorate of potassa, according to the plan recommended by Wöhler [m], and the liquor, after filtration, subjected to the action of sulphuretted hydrogen. Here, as a dirty, greyish precipitate was thrown down, I might have suspected the presence of arsenic, but this presumption was overturned by the application of Marsh's test, which afforded no indications of arsenic whatever. I may also add, that Professor Brodie was good enough to submit another sample of the same turnips to chemical examination in his laboratory, but was unable to detect the presence of arsenic in it.

I also, on the 10th of last July, applied to a plot of similar dimensions with the former, containing a crop of barley, a solution of 4 ounces of nitrate of baryta in 4 gallons of water. The application was repeated on the 18th and 28th

[m] Handbook, p. 218.

of July, and on the 11th, 18th, and 21st of August. No perceptible effect was produced upon the crop so treated, although the amount of grain was reduced from $4\frac{1}{2}$ lbs. in the 100 square feet, to $3\frac{1}{4}$ lbs. This, however, might be an accidental difference: for after a similar application to a crop of turnips, the weight of the roots somewhat exceeded the average of the crop in the rest of the same plot, weighing in 100 square feet 62 lbs., whilst the average of the remainder was 58 lbs.

Here the solution of nitrate of baryta was applied on September 3rd, 10th, 17th, and 27th, without any perceptible influence upon the character of the crop.

At the same time, similar experiments were made upon two corresponding crops of barley and turnips, by the application to them of 10 ounces of nitrate of strontia in 10 gallons of water, the application being renewed five successive times. No perceptible difference in either case resulted from the application in the character of the crop, and its amount in either instance seemed enhanced, but this might perhaps have been owing to other causes.—The following were the results obtained:—

		In 100 square feet treated with strontia.	In 100 square feet not so treated.
Barley	Grain	5 lb.	$4\frac{1}{2}$ lb.
	Straw	16 "	11 "
Turnips	Roots	53 "	40 "
	Tops	13 "	20 "

The barley and barley-straw, the turnip roots and tops, which had been watered with the above two solutions, were separately incinerated, their ashes reduced to whiteness by heat, then treated with nitric acid, and the solution evaporated to dryness and exposed to a red heat. They were then severally dissolved in hydrochloric acid, and filtered. To the filtered solution sulphate of soda was added, and the precipitate formed was washed and dried.

Presuming the latter to consist of the earthy sulphates, I exposed each of them to a red heat for half an hour, mixed with an equal weight of carbonate of soda. The mass was

then digested in water, and to the undissolved residue, muriatic acid was added.

So far the process was the same in all the eight cases; but to the hydrochloric acid solution, derived from the plants that had been watered with nitrate of baryta, after driving off the water, alcohol of specific gravity of ·810 was added. The salt derived from the grain of the barley was wholly dissolved by the alcohol: and was, therefore, inferred to consist entirely of chloride of calcium. That from the barley-straw, as well as those from the turnip-tops and roots, left, it is true, a slight residuum insoluble in the alcohol; but in only one of them, namely, that derived from the turnip-tops, did the residuum, when dissolved in water, yield any precipitate with sulphate of soda; and, therefore, in that alone was the presence of baryta established.

Here then we are drawn to the same conclusion as in the case of the arsenic, namely, that the earth was not taken into the system of the plant, but had merely adhered to its external surfaces.

With regard to the other five solutions, which were derived from the ashes of plants watered with a solution of nitrate of strontia, after dissolving them in hydrochloric acid, and evaporating to dryness the solution obtained, they were severally treated with absolute alcohol like the former.

As, however, this menstruum takes up chloride of strontium as well as chloride of calcium, I trusted for the detection of the former earth to two tests, namely, first—the colour of the flame produced by setting fire to the alcoholic solution; and secondly—the effect of the addition to it of a weak solution of sulphate of soda. Now, in neither of these ways could I obtain any indication of the presence of strontia in the salt derived from the grain of the barley or from the roots of the turnips; but a slight precipitate was produced by the addition of sulphate of soda in the case of the barley-straw and of the turnip-tops, referable, as I conceive, to the same cause as the presence of baryta and arsenic in the foregoing cases, namely, to the adherence of a small quantity of the earthy salt to the external surfaces of the plant to which the solution had been applied.

I have trespassed upon the time of the Society in the detail of these experiments to a greater extent than I should have been otherwise disposed to do, out of consideration for those chemists who have arrived at conclusions opposite to my own; as it seemed but right to give to others ample means of judging how far my experiments are calculated to shake their confidence in the results previously obtained.

The facts, indeed, which I have brought forward in this Memoir are, I must admit, too few in number, and, perhaps, too deficient in the required precision, to settle the question at issue; and I have consequently this very year commenced a similar set of experiments, in which I hope to have avoided the source of possible error which has been pointed out in those already undertaken,—namely, the adherence of some portion of the salt applied to the external surfaces of the stem and leaves of the plant, by introducing the salt into the soil before the seed is sown. I have also substituted the arsenic acid for the arsenious, conceiving that, being isomorphous with phosphoric acid, it has a better chance of being assimilated.

Whilst, however, I abstain from expressing a decided judgment as to the question at issue, I cannot help entertaining a confident belief, that as the results to which my experiments appear to point, seem more in accordance with the general principles of vegetable physiology than those opposed to them, further investigations will establish the fact, that whenever abnormal substances are taken up by a living plant, it is in consequence of some interference with the vital functions of the roots, caused, in the first instance, by the deleterious influence of the agent employed[n].

[n] In stating this as my belief, I do not intend to pronounce what substances are to be regarded as abnormal, either to vegetables in general, or to certain species in particular.

The late investigations, carried on by means of the spectrum, have shewn, that even lithia is normally present in the ashes of tobacco; and, from the experiment of the Prince of Salm Horstmar, alluded to in p. 109, it might be conjectured that this alkali was present in plants generally.

Copper also was detected in them, as early as 1816, by Dr. Mupner, (see Sewheigger's Journal), and by Sarzeau, in eleven different kinds, in 1830. (*Journal de Pharmacie.*) These latter statements, indeed, have been called in

question by Danger and Flandin; but they appear to be substantiated by the later researches of Dr. Odling, recorded in the "Guy's Hospital Reports," 1858.

The latter found copper in flour, in grain, in the straw of wheat and barley, in mangel-wurzel, and in Swedish turnips, as well as in a variety of animal substances. But the quantity discovered in these instances was so minute, as to lend no countenance to the idea, that so large an amount of sulphate of copper as that taken up by plants in the cases reported by Saussure, could have found its way into their organism, if the vitality of the roots had continued unimpaired.

SUPPLEMENTARY NOTE.

(*From the Journal of the Chemical Society, Vol. XV. for 1862.*)

IN a memoir read before the Chemical Society in May, 1861, and since published in the Society's Journal [a], I intimated my intention of continuing the researches begun last year, "On the power ascribed to the Roots of Plants, of rejecting Poisonous or Abnormal Substances presented to them;" and as in the experiments therein detailed, the foreign ingredients had been applied to the plants after they had sprung from the ground, in the liquid form, by watering them with the solution, and it was conceivable that some small portion might have adhered to their external surfaces, even though none had been actually absorbed by the roots, —I this time endeavoured to avoid that source of error, by introducing the salt into the soil before the seed was sown.

The abnormal substances tried were, as before, the earths strontia and baryta in combination with nitric acid,—and the metal arsenic, which, however, was applied, not in the form of arsenious, but of arsenic acid, the latter component being isomorphous with phosphoric acid, and therefore, as was conceived, more capable of being substituted for it in the organism of a plant than arsenious acid, and thus of becoming assimilated.

The crops tried were, as on the former occasion, barley and turnips, and in neither of the two did any very marked difference in the quantity of the crops obtained arise from the application of the salt. If any thing, the advantage seemed to lie rather on the side of those portions of the field which had been thus treated.

The crops were severally examined in the same manner

[a] Vol. xiv. p. 209.

as those of the year preceding had been; but in no one of the six cases, three being the samples of turnips tried with arsenic, baryta, and strontia, and three, those of barley treated with the same substances, did any indications of the poisonous or abnormal ingredient which had been introduced into the soil, manifest themselves in the resulting crop.

I consider these results as rather more conclusive than those obtained in the preceding year, because the substance experimented with, being incorporated with the soil before the seed had been sown, could hardly fail to get into contact with the roots at some part or other.

The only question that can arise is, whether the quantity administered was in each case sufficient to become appreciable in the plants that had grown in contact with it. In the case of the arsenic, I hardly know how this point could be determined more fully, as a larger dose of the poison would be fatal to the vitality of the plant, and thus the very conditions of the problem would be vitiated. With regard, however, to the strontia and baryta, where this objection does not seem to exist, it might be well to settle the point more conclusively, by trying whether the earth would be taken up when quantities of either, larger than were this year employed, had been introduced into the soil.

APPENDIX.

In two lectures[a] given to the University in 1861, on the Physical Forces concerned in the phenomenon of vegetation, and especially on those which form the subject of the Memoirs on Colloid Bodies contributed by the Master of the Mint, I communicated a more popular view of the substance of the two preceding Papers, applying to the explanation of the phenomena of vegetable secretion the principles lately established by the last-named eminent chemist. These Lectures I do not repeat, as they would be in great measure a repetition of what has been already introduced, but some of the concluding passages may perhaps be worth appending, as conveying the general conclusions to which the experimental researches alluded to appear to conduct us.

It follows, then, from these experiments, that no indications of the presence of abnormal substances were afforded, either by any marked difference in the quantity and character of the crops, or by such chemical tests as appeared to me most appropriate for detecting each.

The only alternative, therefore, that seemed to present itself was, either that, as in the case of those normal ingredients which we had before considered, they were absorbed and afterwards excreted, or else that some power resided in the roots, by which they were in the first instance excluded from the vegetable organism.

The former of these suppositions seems to harmonize best with our preconceived notions, and to be most agreeable to analogy; but it is met by the obvious objection, that such substances, if absorbed, ought, like the normal ones not assimilated, to be detected *in transitu* in different parts of the organism.

I have already stated my inability to discover any one of

[a] Since published in the "Gardeners' Chronicle."

the three bodies which I had introduced into the soil in the plants grown in it; but it is possible that others may be more successful, as a Belgian chemist is said to have found zinc in a variety of *Viola lutea* which springs up in soil impregnated with calamine, near Aix-la-Chapelle, and which exhibits evident marks of the influence upon it of some foreign ingredient in the colour of its petals, which are uniformly yellow under such circumstances; whilst on the confines of the zinc rock, where it touches both this and the neighbouring limestone rock, they are only partially so, and become altogether purple at a little distance beyond.

Dr. Odling, too, has substantiated the existence of copper, in flour, in grain, in the straw of wheat and barley, in Swedish turnips, and in a variety of animal substances.

But if we were inclined to disbelieve in the existence of a selecting power in the roots, we should be bound, at least, to admit the same with regard to the secreting organs of a plant, which must in that case have the property of rejecting some bodies, while they assimilate others; and at any rate, the office performed by the spongioles of the roots can hardly be one of so purely passive a character as is believed by some, judging only from the property which has been assigned to them by the observations of Huxtable and Thompson, and which was established by the valuable researches of Professor Way. For it would seem from the investigations of these gentlemen, that whilst the carbonate of ammonia present in rain water and in manures is so firmly fixed in the soil, that, under ordinary circumstances, water containing carbonic acid is unable to withdraw it, the same is nevertheless commonly absorbed and conveyed into the tissue of plants by means of their roots.

Hence, as Liebig observes, vegetables must obtain their nourishment by virtue of some specific attraction exerted by the rootlets upon the ingredients locked up within the earth; a property, wonderful indeed, but perhaps not more so, than that possessed by the leaves of converting carbonic acid into various organic principles through the elimination of oxygen.

Thus, in exploring the recesses of the vegetable no less than of the animal kingdom, we are met at every step with phenomena, which seem to imply something beyond, if indeed not at variance with [b], the forces which operate upon inert matter.

These phenomena it is our business to note and to record, eagerly availing ourselves of every new discovery in physics which may seem calculated to explain them, but at the same time religiously abstaining from any crude hypotheses, which, however they may captivate the imagination, do not carry with them the convictions of the understanding.

I do not consider that we are violating this sage injunction, when we refer these unexplained facts to the vital principle, for such an expression is only meant to imply, that the causes, whatever they may be, which bring about the effects in question, are only found to operate, so long as life continues.

I should, indeed, be far from wishing to prejudge the question, as to whether some or even all of the phenomena alluded to may not hereafter admit of being referred to physical laws, remembering, as I do, that even within my own time it was a ruled point, that no organic compound could be generated except under the influence of the living principle, whereas it is now known (thanks to Wöhler, and others who have followed in his footsteps,) that so many of them can be produced by ordinary chemical manipulations.

At the same time it seems to me, that we are bound, if we wish to maintain our character as philosophers, to discountenance any of those vague analogies [c] derived from the

[b] If I am asked how I reconcile this opinion with the principle laid down that chemical and physical laws are never suspended, I must simply plead my ignorance. If gravitation pervades all nature, if a chemical re-agent acts upon living as well as upon dead matter, I cannot bring myself to believe that the osmotic force is ever absent from the membrane of a cell. In suggesting, therefore, that the roots may have some power of excluding useless or noxious ingredients, I only mean, that such is the inference which the experiments would seem to warrant, and am far from pretending to dictate the mode in which this effect is brought about.

[c] Such, for instance, as that of illustrating the assimilation of normal sub-

contemplation of inorganic bodies, which have been forced into the science of physiology, with a view of elucidating the facts of vegetation, by men who, in their zeal to explain everything on purely materialistic principles, choose to ignore the mysteries which still shroud the approaches to the shrine of the Rhodian Genius [d].

stances by a plant, with the gradual growth of a crystal by the clustering of similar molecules round an existing nucleus.

[d] See Humboldt's little allegory "On the Vital Force, or the Rhodian Genius."

On the Vitality of Seeds;

BEING THE SUBSTANCE OF A LECTURE ON RURAL ECONOMY, DELIVERED AT OXFORD IN 1863.

AMONGST the subjects embraced under the general head of Rural Economy, is the length of time during which seeds are capable of preserving their vitality unimpaired, so as, under favourable circumstances, to germinate and to produce a living plant.

On this the most marvellous accounts have from time to time been put forth, and been even countenanced by respectable authorities.

One of the most authentic, perhaps, of these is the statement, to which Dr. Lindley has given his sanction, as to some Raspberry seeds having been taken from the abdomen of a skeleton found in a coffin thirty feet below the surface of the ground, in a barrow, near Maiden Castle, in Dorsetshire, in which were coins of the Emperor Hadrian, and to which, therefore, a date of 1600 or 1700 years may be assigned. These seeds germinated, and produced living plants, one of which is at this very time growing in the Botanic Garden, at Oxford.

Another, apparently as well established, is one which Desmoulins[a] relates, namely, that in 1834 a number of brick coffins were discovered in the department of Dordogne, containing skeletons, most of which presented this remarkable peculiarity, that the head of the skeleton rested on a heap of apparently well-preserved seeds.

The bricks were of Roman make, and other circumstances led to the conclusion, that they were of the third or fourth century of the Christian era.

Now these seeds in many cases when brought out into the air began to germinate, whilst some of them, even in

[a] Hooker's Companion to the Botanical Magazine, vol. ii.

flower-pots, as well as in a garden, produced plants, viz. of the *Heliotropium europæum*, the *Medicago lupulina*, and in one instance, the *Centauria cyanus*. Sufficient care does not appear to have been taken to prevent other seeds from gaining admission into the soil, but upon the whole the evidence appears pretty satisfactory.

Sir W. Hooker also relates, that the Rev. W. Burroughes, of Hoveton Hall, Norfolk, raised some plants of *Centranthus ruber* (*valeriana rubra* of Linnæus), from seeds taken by himself from an ancient coffin dug up at Wymontham Abbey, in that county. The abbey was founded in 1107, and the coffins were found at the foot of the great altar; one coffin contained the body of a female, and from its position the latter may be conjectured to have been nearly allied to the founder, in which case the seeds would go back to the twelfth century.

Numerous instances are cited, where grain taken from the cases of Egyptian mummies have germinated, and wheat is now commonly cultivated in this country to which the name of mummy wheat has been given, from its having been derived from grain brought from Egypt, and supposed to be obtained from the above sources.

But unfortunately the evidence is for the most part unsatisfactory, the wheat having been generally obtained from the Arabs, on whose authority alone the fact in these cases rests.

Thus Sir Gardiner Wilkinson brought home some grain which he thought he could rely upon as genuine; but when it sprung up, maize—a plant certainly not known to the Egyptians of olden times—was found amongst it.

A similar Report may be seen in the Proceedings of the Ashmolean Society for May 5, 1845, where some seeds said to have been taken from a mummy case in Egypt, and supposed to be grains of wheat, were submitted to Mr. Baxter, sen., our former Gardener, and determined by him to belong to a species of maize. It is curious, however, that in this instance, the grains differed from the common Indian corn, in having a much narrower seed, and a highly developed calyx.

Sir William Hooker, to whom they were also submitted by Dr. Davy, considered them to belong to the variety which grows wild in Paraguay, and which is probably the parent of that which is now cultivated in all the warmer regions of the globe. It differs from the latter in the possession of a proper floral covering, whereas the common maize is altogether destitute of it. It has been particularly described by Aug. St. Hilaire, and by M. Bonifous. The extreme improbability of the seeds of a plant, only known wild in the interior of Paraguay, finding their way into an Egyptian tomb 2000 years ago, compels me to place this also amongst those cases of forgery, which tend to shake our confidence in the general truth of such statements.

I do not see, however, how we can well find fault with the account given by Mr. Tupper in the "Times" of September, 1840, with respect to some seeds of wheat which he had obtained from Sir Gardiner Wilkinson, and which the latter stated, that he had taken with his own hands from the interior of some alabaster sepulchral vases, in an ancient tomb opened by himself in the Thebaid, and probably 3,000 years old. Wilkinson gave the seeds to Mr. Pettigrew, who handed them over to Mr. Tupper.

The latter appears to have shewn great care in sowing them, having himself examined the soil, which he introduced into garden pots, to see whether grains of wheat could have been present, and he sowed three seeds in each of four pots. One of them germinated, the other eleven dying. From the one which grew, two ears came up, yielding altogether 27 grains, and from the latter an abundant crop was obtained; 18 grains in Mr. Mitchell's Nursery Garden, Brighton, having produced 625 ears, very like Talavera wheat of the present day.

Thus, Mr. Tupper says, this plant of wheat may be the product of a grain preserved since the time of the Pharaohs; so that we moderns may eat bread made of corn which Joseph might have thought to store in his granaries, and may almost literally snatch a meal from the kneading-troughs of departing Israel.

These are all the instances of very extended longevity that I have been able to collect, sufficiently authentified to be appealed to; for the bulb of an onion taken from the hand of a mummy, which is said to have vegetated, is too absurd a tale to be credited, and, indeed, tends to throw a doubt upon the analogous statements made with respect to seeds.

If, however, we examine the cases of a less extreme longevity than the above, but still where vitality has been preserved for a considerable time, it will be found that the evidence is more conclusive. Thus Dr. Robert Brown mentions a seed of *Nelumbium,* which germinated after having been preserved in the herbarium of Sir Hans Sloane for nearly 150 years.

Girardin caused seeds of a kidney-bean, which had been taken from Tournefort's herbarium, to germinate, although 100 years must have elapsed since they were gathered.

Davies, (Welsh Botany), mentions seeds of *Datura stramonium,* which vegetated after having been buried more than a century.

Sir C. Lyell, in his Tour in America, reports, that the seeds of *Nelumbium* are believed to vegetate when a century old.

Seeds of the Camomile, taken from a very ancient tomb in Auvergne, are said by M. de Caumont to have germinated, as had also Melon-seeds after remaining for 70 years in an old bureau.

The same inference has been deduced from facts related as to the springing up of plants in soil where they had not been sown, and where the seeds appear to have lain dormant for a very considerable lapse of years.

Dureau de la Malle[b], in his essay on the alternation of Plants, states, that on cutting down a forest at Landres, in the province of Perche, north-west of France, the soil was covered entirely with Broom, *Digitalis, Senecios,* Privet, and Heaths, together with Birches, although the trees of the forest had been Oak, Beech, Chesnuts, and Elms, with Holly and Buckthorn, as shrubs.

In the United States the same thing has often been ob-

[b] *Ann. des Sc. Nat.,* 1825.

served; where firs have been removed, oaks, &c., having sprung up spontaneously; and where an oak forest has been extirpated, firs often taking their place.

Ray relates, that after the great fire of London, *Sisymbrium irio* started up where it had never before been known.

Georgi and Pallas relate, that a pine forest, when entirely destroyed in Russia, is replaced by service-trees, birches, the common viburnum (wayfaring-tree), lime-trees, rubuses, &c., and this statement Von Buch confirms from his observations in Norway.

Mackenzie says, that when a forest of spruce, or of birch, is destroyed by fire, poplars, which had never before been seen, start up in their place.

Hearne assures us that the strawberry is sure to grow wherever fire has passed over a country.

Auguste St. Hilaire states, that in Brazil, when a road is cut through a native forest, trees quite different from those which the forest contained have been observed to spring up, ferns and other herbaceous plants of a new kind at the same time making their appearance.

In the Isle of France, when a clearage is made in a forest, the soil is instantly covered with species quite alien to the soil, and indigenous to Madagascar; but the most abundant plant of any, is a kind of raspberry, a native of the Moluccas.

Similar statements are given with respect to our own country. Thus Miller noticed *Plantago psyllium*, a plant of southern Europe, growing in a ditch which had been newly cleaned, and where it had never been known to grow since the memory of man.

The Rev. Vernon Harcourt mentions a case where turnip-seed must have retained its vitality for at least eight years. A field had been prepared for white carrots, and the carrots were sown, but the season being very dry, they did not grow. The ground was, however, soon covered with a crop of white turnips, which had been planted eight years before, but of which the seeds had been carried down by rains below the ordinary depth of ploughing, and therefore beyond the reach of atmospheric influence.

To these facts we may add others in which seeds were

found several feet under ground, and which yet germinated. This was the case with the corn marigold, which was found in Stirlingshire under six or seven feet of peat. The same appears to be the case with white clover, which starts up frequently when new land is turned up.

In Newfoundland, after a fire in the forests, the ground became covered with a luxuriant growth of raspberry shrubs, succeeded by a thick wood of birch, although previously to the fire nothing but spruce and fir had been seen for miles.

Mr. Kemp took up some sand from a pit 25 feet from the surface, and found that plants of polygonum, convolvulus, *Rumex acetosella,* and a variety of *Atriplex patula,* sprang up from it. Now he contends, that the seeds of these plants must have been deposited at a time when a lake was formed in the course of the Tweed, about a quarter of a mile from Melrose, and as no lake existed in the time of the Romans 2,000 years ago, he concludes, that the seeds must have been of still greater antiquity than the above date.

Still more worthy of attention are the statements put forth by so accurate and exact an observer as Mr. Darwin. He mentions a case where charlock, (*Sinapis arvensis,*) sprang up spontaneously in a field in which none had been sown within nine years at least; and where, therefore, they must have lain in the soil during that period with their powers of germination unimpaired.

A nurseryman from Hammersmith also states, that having in the year 1823 sown some seed of sea kale, nineteen years afterwards, upon burning the soil, the same crop sprang up spontaneously.

Mr. Doubleday, of Epping, relates the same with respect to the *Lavatera arborea,* which, having been allowed to seed in 1836, continued to come up for several years afterwards.

With regard to the spontaneous growth of charlock also, many similar cases to that of Darwin are recorded.

Even moisture does not seem speedily fatal to the longevity of seeds in certain cases.

Dr. Masters, the well-known editor of the "Gardeners'

Chronicle," informed me that he observed at our Botanic Garden, "in the early part of 1854, some seedling plants of gorse, (*Ulex europæus,*) which had come up in the soil in which the *Victoria regia* had been grown the year before. The earth had thus been covered, for several months, by water of a temperature probably never lower than 50°, and sometimes reaching to 80° Fahrenheit. It might have been expected, that any seeds that the soil contained would have rotted during such prolonged submersion, but such was not the case. When the water was removed from the tank, and the mound of soil in the centre became tolerably dry, several seedling plants were observed by Mr. Baxter, who called his attention to them. The seeds that he examined did not penetrate deeply into the soil, but must have been near the surface during the time that the water was in the tank, because the soil had not been moved. Else it might be said that the seeds had been at the bottom of the mound, and thus their vitality had been preserved, till called into activity by the removal of the water, and the disturbance of the mould, which would have brought them to the surface. No fresh soil had been added, nor could he discover any other probable means whereby the seeds could have been introduced after the mould had been exposed to the air."

Setting aside, however, those cases where the longevity ascribed to the seeds by no means exceeds what has been ascertained by direct experiment, the evidence, as we shall see presently, does not appear to be altogether unexceptionable.

In the case, for instance, of seeds found at a certain depth below the surface, how can we be sure that they may not have been washed into the spot where they are found, at some period subsequent to the deposition of the soil which covers them?

In the instances of distinct genera of plants springing up spontaneously after the destruction of a forest, how do we know that the seeds may not have been wafted from a distance, and established themselves in a soil, which, owing to

the long continuance of other sorts, proved to be congenial to them?

Where, as in the case cited from the Isle of France, plants spring up that are not indigenous, the difficulty is great whatever hypothesis we assume, but it is just as easy to suppose the seeds to be wafted across at the time, as to have been dormant in the soil for an indefinite period, since even then they must originally have been conveyed from their native country in some manner or other.

The most conclusive evidence hitherto adduced, would seem to be derived from those instances, where seeds, like those mentioned by Desmoulins, brought from an inclosed place, such as a tomb, to which a certain antiquity is fairly assignable, have actually germinated; but even here sufficient pains does not appear to have been taken, to prevent the possibility of other seeds gaining admission from without.

Much uncertainty, as we have seen, hangs over the case of the mummy wheat of Egypt, especially when it is recollected how often, where seeds really taken from a mummy case have been sown, they have altogether failed, as I can testify from my own observations. Even the remarkable instance of the raspberry seed mentioned by Dr. Lindley, resting, as it does, upon the authority of a single individual, has, I believe, been disputed; and it is to be remarked, that only one of twelve seeds experimented upon by Mr. Tupper is reported to have germinated.

The love of mystification, indeed, which prevails so generally amongst mankind, no less than the pleasure we instinctively feel at hearing any extraordinary fact, and the difficulty we therefore experience in rejecting it, are circumstances which must always be taken into account, in estimating the probability of any such facts as those which we are now discussing.

Nevertheless, after making due allowance for all these drawbacks, most persons perhaps will be inclined to admit, that the preponderance of evidence is in favour of the opinion that the vitality of certain seeds has been prolonged for a very considerable period; nor indeed can we reason-

ably dispute, that a century or two will fail to extinguish life in certain species, when we read the statements of Dr. Brown with respect to the seed of the *Nelumbium*, confirmed by the report communicated by Sir C. Lyell with respect to the same seed in its native country, Virginia; the similar report given by Dr. Masters as to another seed germinating which he had taken from this same herbarium, or to the equally well established instance given by Tournefort with respect to a kidney-bean taken from his own herbarium. Why, indeed, should we deny such facts as these, when we are told by Decandolle that a bag of seeds supplied the *Jardin des Plantes* annually with sensitive plants during sixty years; when we learn that seeds of camomile have germinated after seventeen years, those of colza and *Malva crispa* after eighteen, *Althæa rosea* after twenty-three, maize after thirty, French-beans after thirty-three, and melons after forty-one, and even in the instance above recorded after seventy. The distance of time between the latter and the ones previously alluded to, is not great enough to raise any reasonable doubt upon the subject.

It must be confessed, however, that there is a wide interval between the most distant of these authenticated cases, and the longevity ascribed to the mummy wheat of Egypt, or the raspberry seeds of Dorsetshire; whilst even that is nothing to what we must ascribe to them, if we were to avail ourselves of this principle as a means of accounting for the preservation of plants during those long periods of cold which geologists suppose to have preceded the more genial climate of the present time, and which, therefore, would suspend for a season of indefinite, but to us immense duration, the vegetation which prevailed both after and before it. Now the durability of a seed will be dependent upon the susceptibility of the matters which constitute its albumen to undergo change. When the latter is of an oily nature, as in the coffee, the magnolia, clove, &c., the seeds must be sown immediately upon being ripe. For this reason so many seeds conveyed from a tropical climate lose their vitality, the oil becoming rancid, and undergoing certain chemical changes which destroy the life of the seed.

But in other cases, the seed is preserved unaltered in consequence of the carbonaceous matter which exists in it resisting decomposition, and in that case there is no saying how long vitality may be maintained, provided the circumstances under which the seed is placed are not such as to affect the carbon present in it.

For the first step in the process of germination is simply a chemical one; it is the removal of a portion of that redundant carbon which appears to have been introduced for the express purpose of preserving the seed unchanged, and which, uniting with oxygen, goes off in the form of carbonic acid gas. This done, the albuminous portion of the seed passes readily into a soluble form, the starch being converted into gum and sugar, and being thus rendered suitable for the nourishment of the embryo.

Under these circumstances whatever vitality exists in the embryo itself begins to shew itself, and the result is, the evolution of the plumula, or young stem; of the radicle, or young root; and of the cotyledons, or the rudimentary leaves.

Thus, whilst on the one hand it is difficult to affix a limit to the vitality of a seed, it must be admitted on the other, that the maintenance of its life is dependent upon so many conditions, that it can rarely happen, but that within a certain period, varying according to the nature of the seed, something will destroy it. Warmth on the one hand, dampness on the other; a too liberal supply of oxygen on the one hand, the entire absence of it on the other; together with sundry other external circumstances which perhaps are less fully ascertained, may all tend to bring about those chemical changes in the seed with which the maintenance of its vitality is incompatible.

Some seeds will only germinate after they have been long boiled in water, the outer coat requiring to be first softened, before the radicle can push its way through it. So far, therefore, as experiment and observation have hitherto gone, we know of nothing, which absolutely renders it impossible for seeds to retain their vitality for an indefinite period, except it be, the entire exclusion of air.

For if this be the case, it would seem to follow, that some change, however slow, is taking place in all seeds connected with the absorption of oxygen, and consequently that their preservation can only extend to a limited period.

But further experiments are wanting to determine this point, and in the present state of our knowledge there is nothing certainly in the physiology of the seed to lead us to assign a definite limit to its vitality.

I now, then, proceed to give an account of the direct experiments which have been instituted to determine the longevity of different kinds of seeds.

Of these, I know but of two series as yet undertaken, the one by M. Alphonse Decandolle at Geneva, the other under the auspices of the British Association by the late Mr. Strickland, Professor Henslow, Dr. Lindley, and by myself.

Decandolle, in the *Ann. des Sc. Nat.* for Dec. 1846, informs us that he had experimented upon 368 species, all of which were fifteen years old; of each of these twenty seeds were sown, and the result was, that out of the whole number only seventeen species germinated, which all belonged to the natural families *Malvaceæ*, *Leguminosæ*, and *Labiatæ*; the two first greatly exceeding the last in this respect, as out of thirty species of *Labiatæ* only one germinated, whereas out of ten of *Malvaceæ* five, and out of forty-five of *Leguminosæ* nine came up.

On the other hand, no seed belonging to the natural families *Scrophularineæ*, *Umbelliferæ*, *Caryophylleæ*, *Gramineæ*, *Cruciferæ*, or *Compositæ* retained their vitality after seventeen years. In one instance only, *Dolichos unguiculatus*, did more than half the number of seeds vegetate.

Decandolle suspects that small seeds lose their vitality more quickly than larger ones, owing, perhaps, to the greater surface exposed to the action of the air upon them in the former case than in the latter.

The experiments carried on under the auspices of the British Association extended to 288 genera, and 71 natural families, including nearly all the kinds of vegetables culti-

vated for culinary and other domestic purposes, and 100 seeds of each kind were in general sown.

If any of these germinated, a similar number of the same were experimented upon again after a lapse of five years, and so on as long as any came up. In this way it was found that the greater number of species had lost their vitality altogether after being kept ten years.

It was, however, ascertained that no less than thirty-four species, or about one-seventh of the whole number, retained their vitality after ten years; twenty species, or about one-fourteenth, after twenty years; but that the only species that reached twenty-five, twenty-six, or twenty-seven years, belonged to the natural families, *Leguminosæ, Malvaceæ,* and *Tiliaceæ.*

Two only, both *Leguminosæ,* maintained their vitality after forty years.

Thus the results arrived at, are in general conformable with those of Decandolle; no cruciform plant, only one belonging to the great family of *Compositæ,* and, what is remarkable, none of the *Gramineæ,* having been found to retain their vitality after ten years.

In both sets of experiments, the greatest instances of longevity were found amongst the *Leguminosæ,* which are destitute of albumen; whilst the *Umbelliferæ,* which contain an essential oil, seemed to lose vitality particularly soon.

We can, therefore, better understand how it is that white clover makes its appearance spontaneously; that a cruciform plant, like the *Sisymbrium irio,* should have started up after the great fire of London; or why charlock should be so common a weed in cases where none of it can have been sown for years before.

Our experiments, however, were not entirely confined to seeds of this comparatively fresh date. A considerable number were likewise obtained from old herbaria, especially from those preserved at our Botanic Garden, but in no case did any one of them come up; a fact, not of course impugning the correctness of the positive statements given on the contrary side by others, but only tending to shew, that the latter were exceptional cases, and consequently indicate

nearly the extreme point to which the vitality of the seeds can be supposed to extend.

The enquiry, however, originally proposed to be instituted embraced a much wider range than this, as it was proposed to collect samples of ancient soils from situations where from their depth vegetation cannot be now taking place, and, by exposing them to air, light, warmth, and moisture, to ascertain whether any, and if any, what species of plants will spontaneously vegetate in them.

Thus it was proposed to examine, rocks of ancient deposition taken from a considerable depth, alluvions of rivers, accumulations of peat, surface soil buried by landslips or by volcanic eruptions, as well as ancient tumuli, encampments, the soil of graves, wells, &c.

We have been indebted to Sir Walter Trevelyan for one sample of the above kind; but it is to be regretted that in general this part of the investigation has been neglected. In none, however, of the few cases that have yet come under our observations, did any seeds come up under the circumstances above alluded to.

With respect, indeed, to the series of experiments already instituted, a question will naturally arise, whether the circumstances under which the seeds had been placed, were as favourable to their preservation as any that exist in nature. On this point, no doubt, authorities will differ, but after consulting with Dr. Lindley and others, it appeared to me that the best expedient would be to place them in porous earthen jars, covered over with brown paper, and kept in a dry place not artificially heated.

Thus air was not entirely excluded, and if any slow process of oxidation took place in the seeds, its products would be removed, and in particular that humidity, which the seeds might themselves, notwithstanding careful drying, retain, would evaporate, instead of producing, through its retention, a slow fermentation in the mass.

A similar method has been more successful than any other in transmitting seeds to and from India[c].

[c] Gard. Chron., 1846, p. 51.

When they are of an oily nature, indeed, such as those of the chesnut, filbert, and the like, it has been found best to envelope them in wax, and in this way they have arrived at Sherampore, Bombay, and Calcutta, in a condition fit for germination. The same method, also, has been adopted with success in the case of bulbs, as well as of the cuttings of forest trees.

Other seeds have been found to travel best, when simply placed in a proper state of dryness, in canvas bags suspended in the ship's cabin, in as cool a situation as possible, and with a free circulation of air; without which precautions they would lose their vitality during the voyage.

APPENDIX.

The following suggestions for experiments on the conservation of Vegetative Power in Seeds, were drawn up by my late oolleague, Professor Strickland.

THESE Experiments are intended to determine the following questions:—

1. What is the longest period during which the seeds of a plant under any circumstances can retain their vegetative powers[a]?

2. What is the extent of this period in each of the natural orders, genera, and species of plants? and how far is it a *distinctive* character of such groups?

3. How far is the extent of this period dependent on the apparent characters of the seed; such as size, hardness of covering, hardness of internal substance, oiliness, mucilage, &c.?

4. What are the circumstances of situation, temperature, dryness, seclusion from the atmosphere, &c., most favourable to the preservation of seeds?

To answer these questions satisfactorily will require the accumulation of a large mass of facts; and although there are many difficulties in the way of such an investigation, and many years may elapse before it can be brought to maturity, yet it is desirable that the British Association should commence the collection of materials for the purpose. It is proposed, then, to invite Botanists and others to undertake the following series of experiments, and to communicate the results to the British Association.

These experiments are either Retrospective or Prospective.

[a] See Report, 1850.

A. Retrospective Experiments.

1. By collecting samples of ancient soils from situations where vegetation cannot now take place, and by exposing these soils to air, light, warmth, and moisture, to ascertain whether any, and if any, what species of plants spontaneously vegetate in them.

> N.B.—Care must of course be taken, that no seeds obtain admittance into these soils from external sources, such as the air or water introduced to promote vegetation.

These ancient soils are either *natural* or *artificial* deposits.

The *natural* deposits belong either to *past* geological periods or to the *recent* period.

a. The deposits of past periods are either secondary or tertiary.

> N.B.—There seems every reason to believe, that the age even of the latest of these deposits is far beyond the maximum period through which vegetative powers can be preserved: yet as many accounts are recorded of seeds vegetating spontaneously in such soils, it would be well to set these statements at rest by actual experiment.

In such experiments state the formation, and describe the geological phenomena of the locality, together with the depth from the present surface at which the soil was obtained.

b. Natural deposits of the recent period may be classed as follows:—

Alluvions of rivers.
Tidal warp land.
Shell marl.
Peat.
Surface-soil buried by landslips.
Ditto ditto by volcanic eruptions.

In these cases state the nature of the soil, the depth from the surface, &c.; and especially endeavour to obtain an approximate date to each specimen of soil, by comparing its depth from the surface with the present rate of deposition,

or by consulting historical records. It would be well to submit to experiment a series of samples of soil taken from successive depths at the same locality.

c. Artificial deposits are as follows :—

Ancient tumuli.

Ancient encampments.

The soil beneath the foundation of buildings.

The soil with which graves, wells, mines, or other excavations have been filled up.

Ridges of arable land, &c.

In these cases state, as before, the depth from the surface, and ascertain from historical sources the approximate age of the deposit.

2. By trying experiments on actual seeds which exist in artificial repositories. These are,—

Seeds in old herbaria and botanical museums.

Seeds obtained from mummies, funereal urns, at Pompeii, Herculaneum, &c.

Dated samples of old seeds from nurserymen and seedsmen.

In these cases, state the circumstances in which the seeds have been preserved, and their date as nearly as it can be ascertained.

B. Prospective Experiments.

In this department of the inquiry, it is proposed to form deposits of various kinds of seeds under different conditions, and to place a portion of them at successive periods under circumstances calculated to excite the process of vegetation. In the case of certain species or families of plants, it would perhaps require many centuries to determine the limit of their vegetative powers, yet it is probable that a very few years would suffice to fix the maximum duration of the greater number, and that many interesting results might thus be obtained even by the present generation of botanists. It is proposed then to form a collection of the seeds of a great variety of plants, (including, wherever it is possible, at least one species of every genus,) and to pack them up (carefully labelled) either alone, or mixed with various ma-

terials, as sand, sawdust, melted wax or tallow, clay, garden mould, &c. in various vessels, as glass bottles, porous earthen jars, wooden boxes, metal cases, &c., placed in various situations, as under-ground, in cellars, dry apartments, &c. At certain intervals increasing in extent,—say at first every two years, then every five, every ten, and, at the lapse of a century, every twenty years, a small number (say twenty) of each kind of seed, from each combination of circumstances, to be taken out and sown in an appropriate soil and temperature, and an exact register kept of the number of seeds which vegetate compared with those which fail.

Should it appear desirable for this project to be carried out by the British Association, they might most effectually accomplish it, by committing a collection of seeds, formed on the above plan, to some qualified person, whose duty it should be, in consideration of a small annual stipend, to take charge of them, and at stated periods to select portions for experiment, keeping an accurate register of the results.

In this manner it is believed, that in regard to the large majority of plants, the limit of their vegetative durability would be determined in a very few years, and that a large mass of vulgar errors on this subject, which now pass current for facts, would be cancelled and exploded.

N.B.—The most effectual way of exciting vegetation in seeds of great antiquity, is to sow them in a hot-bed, under glass, and in a light soil moderately watered.

An annual Report of the experiments conducted according to the above plan was presented to the British Association at each of their meetings from 1841 to 1857, when, a general summary of the results having been presented, it was published in p. 43 *et seq.*, of the volume of this latter year.

Memoir on the Rotation of Crops, and on the Quantity of Inorganic Matters abstracted from the Soil by Various Plants under different circumstances.

Selected by the Royal Society as the Bakerian Lecture, and Published in the Philosophical Transactions, for 1845.

This Memoir, which occupies altogether seventy-three pages, consists of three parts. In the first of these is stated the quantity of produce obtained from equal plots of ground set apart for the experiments in the Botanic Garden at Oxford during ten consecutive years. In the second, the chemical composition of certain crops cultivated in these plots, and the amount of inorganic principles abstracted by them from the soil during the period the experiments were conducted, are severally stated. In the third, the chemical composition of the soil in which the crops were grown, and the proportion of its ingredients that was available for the purposes of vegetation, are detailed.

In Part I. is given a report of the produce obtained from equal portions of the same soil, in eighteen different instances, when the crops were shifted each year from one plot to another, as compared to that in which the same crop was repeated for ten successive years upon the same ground, no manure having been in either instance applied during the prosecution of the experiments. The results proved, on the one hand, that although, owing to the continual draft of nutritious matters from the soil during the ten years, a gradual decrease in the amount of produce was in almost every instance observed, yet that in general this falling off was greater, when the crop was repeated year after year in the same plot, than when it was shifted from one to another. Nevertheless the difference in the amount of produce between the permanent and the shifting crop was not such, as to afford any countenance to the theory proposed by Mons. Decandolle, as to the poisonous nature of root-excretions, except indeed in one in-

stance, namely, that of *Euphorbia lathyris*, a plant with acrid juices, where the ground appeared to be incapable after three years of bearing any further crop; although, when sown afterwards with peas, barley, and beans, a tolerable yield was obtained.

In the other seventeen cases we had to seek further for the cause of the inferiority of the permanent to the shifting crop. Accordingly, in the second part of this memoir, the true reason of the falling off of the crops was traced to a deficiency in the mineral ingredients required for the purposes of the plant, as was proved by their analysis.

And in the third part, the explanation of this deficiency was shewn to be attributable, not to an actual exhaustion of the mineral ingredients present in the soil, but to the consumption of that portion of them which was in a condition available for the purpose of assimilation. It is this latter portion of the subject alone which it is my intention to bring before the readers of this volume, referring them to the original memoir for the details of the amount of crop, and of the mineral ingredients which entered into its composition, under the different circumstances in which the plants were placed.

WHEN we consider the nature of a soil in an agricultural point of view, or in reference to its suitableness for the growth of various kinds of vegetables, two questions naturally come before us; namely, what amount of ingredients capable of being assimilated in the course of time by the crops does it contain; and, secondly, what is the amount of those, which are present in a condition to be actually available for their purposes, at the precise moment when the examination is undertaken.

Both the above points are obviously quite distinct from that relating to the total amount of ingredients which exist in it, and hence some might be disposed to add to the labour of the two preceding investigations, that of ascertaining the whole of its constituents, whether in a state to be affected by the ordinary agents of decomposition, or not.

The latter question, however, seems to me to possess, with reference to the agriculturist, only a speculative interest, and when introduced into a Report intended for his use, may be more liable to mislead than to instruct, unless due caution

be taken to point out to him, how much of each ingredient is to be regarded as inert, and how much of it as applicable to the future or present uses of the plant.

Let us take the case of a natural soil, composed of certain kinds of disintegrated lava, or even of granite, in which it is evident, that an actual analysis, conducted by means of fusion with barytes, or lead, or by any of those other processes which chemists employ for decomposing compounds of a refractory nature, would detect the presence of a large percentage of alkali, not improbably of a certain amount of phosphate of lime, and in short of all those ingredients which plants require for their support, in sufficient abundance. Nevertheless, land of this description, in consequence of the close union of the elementary matters of which it consists, and the compactness of its mechanical texture, might be as barren, and as incapable of imparting food to plants, as an artificial soil composed of pounded glass is known to be, notwithstanding the large proportion of alkali contained in it.

Thus I have myself observed[a], that the soil which covers the serpentine rock of Cornwall, although the latter is principally made up of a mineral consisting of—

Silica	43·07
Magnesia	40·37
Alumina	0·25
Lime	0·50
Oxide of iron	1·17
Water	12·45—HISINGER.

contains, nevertheless, so minute a proportion of magnesia, that in an analysis of a small sample its presence had been altogether overlooked by me, in so great a degree may the mechanical condition of the components, and the state of combination subsisting between them, preserve a rock from the decomposing action of the elements which tend to set loose its treasures.

Now it seems obvious, that whatever cannot be extracted from a soil by digestion in muriatic acid during four or five

[a] Lecture on the Application of Science to Agriculture, from the Journal of the Royal Agricultural Society of England, vol. iii. part i.

successive hours, must be in such a state of combination as will render it wholly incapable of imparting anything to a plant, for such a period of time at least as can enter into the calculations of the agriculturist; and, moreover, that all which muriatic acid extracts, but which water impregnated with carbonic acid fails in dissolving, ought to be regarded as at present contributing nothing, although it may ultimately become available for its purposes.

I have therefore thought proper to distinguish between the immediately available resources of the soil, and those ultimately applicable to the uses of the plant, designating the former as its *dormant*, and the latter as its *active* ingredients.

The portion dissolved after digestion in muriatic acid will contain both the *dormant* and the *active;* that taken up by water impregnated with carbonic acid will consist merely of the latter; the difference in amount between the two will therefore indicate the *dormant* portion of its contents.

The *dormant* and *active* portions may both be comprehended under the designation of its *available* constituents, whilst those which, from their state of combination in the mass, can never be expected to contribute to the growth of plants, may be denominated the *passive* ones.

Every soil which is capable of yielding an abundant crop of any kind of plant after fallowing, must be assumed to possess in itself an adequate supply of all the ingredients necessary for its support in an *available* condition; but it is plain that these could not have existed in an *active* one, or such an interval of rest would not have been required for rendering them efficient.

Accordingly it is quite possible, that after ten years' cropping, the soil of my experimental garden might still retain plenty of alkaline salts and phosphates, although what was ready to be applied to the uses of the plant had for the most part been absorbed by the crops previously obtained.

With a view then to this branch of the inquiry, I first ascertained the nature and amount of the ingredients separable from a given weight of the soil by means of muriatic acid, and secondly, those obtained from the same by a definite

quantity of water impregnated with carbonic acid gas. By a careful analysis it was ascertained that the soil of the Botanic Garden at Oxford contained, within an area of 100 square feet, and a depth of 3 feet from the surface, 3·5 lbs. of phosphoric acid, 6·9 lbs. of potass, and 2·9 lbs. of soda, all in a state to be separated from the general mass by muriatic acid.

That the above, however, were for the most part in a *dormant* condition, appeared from the much smaller amount of the same which could be extracted by water containing carbonic acid, for it was found that of alkaline sulphates [b] not 11 lb. could be procured by these means, whereas

6·9 lb. of potass would have formed	12·7
2·9 lb. of soda	6·5
Together .	19·2 lb.
Extracted by carbonic acid water .	11·0 ,,
Difference .	8·2 ,,

and that of phosphate of lime only 7134 grs., or less than 14 ounces were obtainable; whereas 3·5 lbs. of phosphoric acid, equal to near 7·0 lbs. of phosphate of lime, had been taken up by muriatic acid from the same.

By operating in a similar manner upon soils of the same quality as the above, which had been exhausted by several years' previous cropping, it appeared, that whilst the amount of the ingredients alluded to as *dormant* in the soil did not much vary in the two cases, that of the *active* ones was beyond all comparison greater in the sample of unexhausted soil.

This will appear from the following table :—

[b] The alkalies were estimated as sulphates, as it was found convenient to unite them with sulphuric acid, in which state they admitted of being heated and weighed without incurring loss.

Table of the Quantity of Alkaline Sulphates and Earthy Phosphates extracted by means of Water impregnated with Carbonic Acid from the Soils enumerated below.

Soil examined and treated with water.	Quantity of water added.	Quantity of alkaline sulphate obtained.	Nature of the alkali.	Quantity of alkaline sulphate per quart of water.	Quantity of alkaline sulphate in 1 lb. of soil.	Quantity of alkaline sulphate in 100 square feet (24,600 lbs.) of the soil.	Quantity of earthy phosphate taken up.	Quantity of earthy phosphate per quart of water.	Quantity of earthy sulphate per lb. of soil.	Quantity of earthy phosphate in 100 square feet of soil. (24,600 lbs.)
	qts.	grs.		grs.	grs.	grs.	grs.	grs.	grs.	grs.
From the contiguous garden, first time	2	5·2	Potass.	2·6	...	...	0·7	0·35		
From the contiguous garden, second time	2	7·8	Potass.	3·9	...	...	0·7	0·35		
From the contiguous garden, third time	1	3·4	Potass.	1·7	...	...	0·05	0·05	0·29	7134
From the contiguous garden, fourth time	1	2·6	Potass.	1·3	3·4	83,640				
From the permanent bed of Barley .	2	0·6	Soda.	0·30	0·12	2,950	0·30	0·15	0·06	1470
From the permanent bed of Potatoes .	2	0·7	Soda.	0·35	0·07	1,700	0·25	0·125	0·05	1200
From the permanent bed of Hemp .	2	0·6	Soda.	0·30	0·12	2,950	Scarcely appreciable.			1470
From the permanent bed of Flax . .	2	0·5	Soda.	0·25	0·10	2,450	Scarcely appreciable.			2940
From the permanent bed of Turnips .	2	0·6	Soda chiefly.	0·30	0·12	2,950	0·30	0·15	0·06	3180
From the permanent bed of Beans .	2	0·5	Soda.	0·25	0·10	2,450	0·60	0·30	0·12	4900
From the shifting bed of Barley . .	2	0·7	Soda chiefly.	0·37	0·07	1,700	0·065	0·0325	0·013	3420
From the shifting bed of Potatoes .	2	1·0	Soda chiefly.	0·50	0·20	4,900	0·100	0·050	0·020	4900
From the shifting bed of Hemp . .	2	1·0	Soda chiefly.	0·50	0·20	4,900	Scarcely appreciable.			
From the shifting bed of Flax . . .	2	0·3	Soda chiefly.	0·15	0·06	1,470	0·7	0·35	0·14	3420
From the shifting bed of Turnips . .	2	3·6	Potass.	1·8	0·72	17,700	0·9	0·45	0·18	4410
From the shifting bed of Beans . .	2	1·0	Soda.	0·50	0·20	4,900	0·30	0·15	0·06	1470

From these facts, and from others stated in the course of my memoir, I have conceived myself warranted in deducing the following conclusions:—

1st. That it is quite consistent with the general tenor of the preceding facts and observations, to maintain with Boussingault, that the falling off of a crop is dependent as well upon a deficiency of organic matter proper to promote the nutrition of the plants, as upon a failure of its inorganic principles; not indeed that the organic matter enters, as such, into the constitution of the vegetable, but that by its decomposition it furnishes it with a more abundant supply of carbonic acid and ammonia, which supply accelerates the development of its parts, and thus at once enables it to extract more inorganic matter from the soil, and enables the soil to supply it more copiously with the principles it required for its nutrition.

Hence, perhaps, in part, the advantage of intercalating the *Leguminosæ* and other fallow crops, which generate a larger amount of organic matter than the *Cerealia*, and which thus serve to enrich the soil by what they leave behind them.

2ndly. That it by no means follows, because a soil is benefited by manuring, even though that manure may, as in the case of bones, guano, &c., derive its efficacy from the phosphates it supplies, that it is therefore destitute of the ingredient in question, since it may happen that it possesses abundance of it in a *dormant*, though not in an immediately *available*, condition.

In these cases, in which the agriculturist has been assured by the results of actual analysis, that there is no real dearth of the principles essential to his crops in the soil under cultivation, but where he has ascertained, either by the chemical mode pointed out, or by an experience of the good effects brought about by manures, that the substances in question are not in a state to become immediately applicable to the purposes of vegetation, three courses appear to be open to him:—

1st. To apply a sufficient quantity of the same materials in a state in which they can be absorbed by the plants without delay; 2ndly, to allow the ground to remain fallow, by

which expedient time is given for a further decomposition of its materials, and for a renewed extrication of its useful ingredients, to take place; 3rdly, to produce, by the various methods in daily use, such a stirring and pulverization of the ground, as may admit of a more thorough admission of air and moisture, and consequently accelerate the process of disintegration in a greater degree than would take place under natural circumstances.

Examples will occur to every one of the successful adoption of each of these three practices: of the first, in the ordinary process of manuring, and especially in the beneficial consequences resulting from the use of bones in the exhausted pastures of Cheshire and other similar localities; of the second, in the system so general in the early stages of agriculture, that of allowing land to remain at rest for a certain period with a view of restoring to it its exhausted powers,—a method which would be absurd, if the alkalies, phosphates, and other of the more scanty ingredients were absolutely wanting, but which would be likely to prove efficient, if they were only locked up within the recesses of the soil, and required time to call them into activity; of the third, in the practice resorted to by Jethro Tull, who boasted that he could realize an abundant crop year after year without manure, provided the ground were sufficiently stirred and broken up,—a statement which seems confirmed, by some of the results of spade husbandry, and in a certain degree by those detailed in this paper, with respect to the permanent crops which are herein mentioned as having been made the subjects of experiment.

The choice between the above three methods will of course be determined in each instance by a balance of economy; and although in general this latter consideration will incline the farmer to prefer the ordinary method of manuring, either to the sacrifice of a year's produce, as in the second method, or to the expenditure of labour required to put into practice the third, still there may be cases where it might better answer his purpose to resort to one or other of them, as being more advantageous in itself, or else more suitable to the circumstances of his case.

At any rate it may be important for him to be assured, that at the very time he is ransacking the most distant quarters of the globe for certain of the mineral ingredients required for his crops, he has lying beneath his feet in many instances an almost inexhaustible supply of the same.

For there seems no reason to doubt, that the whole mass of rock, which constitutes the subsoil in the secondary and tertiary districts of this country, is nearly as rich in phosphates and in alkalies as the vegetable mould derived from its decomposition; and although the soil, in which the experiments in my garden were conducted, possessed a depth perhaps three times as great as the average of those in which farm produce is generally raised, yet, on the other hand, the amount of phosphates and of alkaline ingredients reported to be present in the latter appears in many instances greater than that determined in the case before us.

Thus Dr. Ure[c] gives an analysis of a soil in the parish of Hornchurch, Essex, which contained four grains of phosphate of lime in 1000 grains; whereas, of ours, the same quantity yielded little more than one-fourth of a grain; and if the former be regarded as an exceptional case, I might have referred to Sprengel, who states that the percentage of phosphoric acid in the soils he analyzed varied from 0·024 to 0·367; and in the subsoils from about 0·007 to 0·2.

I detected many years ago phosphate of lime in several secondary limestones chiefly taken from the oolitic formation, and Mr. Schweitzer of Brighton has determined the proportion of that ingredient in the chalk near Brighton, to be not less than one grain in the 1000. We need not therefore resort to South America for bones, if means could be found for extracting this ingredient economically from the rocks of our own country.

3rdly. These facts place in rather a new light, although one, it is conceived, not less striking than before, the importance of taking care of the various excrementitious mat-

[c] Journal of the Royal Agricultural Society.

ters at our disposal, whether proceeding from animal or from vegetable sources.

Such substances, indeed, contain the products which nature has, with so large a consumption of time, and by such a number of complicated operations, elaborated from the raw material contained in the soil, and has at length brought into the condition in which they are most soluble, and therefore best fitted to be assimilated by the organs of plants.

To waste them is, therefore, to undo what has been expressly prepared for our use by a beautiful system of contrivances, and to place ourselves under the necessity of performing, by an expenditure of our own labour and capital, those very processes which nature had already accomplished for us, without cost, by the aid of those animate or inanimate agents which she has at her disposal.

4thly. The analyses above reported may suggest caution as to the inferences which some might be disposed to deduce from certain researches lately announced, with respect to the power which a plant possesses of substituting one alkali, or one earth, for another in the processes of vegetation.

This substitution, indeed, however brought about, is a fact which hardly admits of being questioned, supported as it is by the testimony of men so eminent as Saussure and as Liebig; and indeed many of the analyses detailed by me in the Philosophical Transactions might be appealed to in corroboration of its truth.

Thus we find, that whilst the amount of bases agreed pretty nearly in the three crops of the same plant which had been analyzed, the proportions between them often varied considerably. This is particularly seen in the case of the lime and magnesia, the deficiency in one of these earths being often made up by an excess in the other.

In like manner a deficiency of potass is found to be compensated by an increased amount of soda, and the same remark seems to apply to the acids.

Still we have not as yet sufficient data for determining to what extent this exchange of the usual ingredient for an-

other can take place; whether indeed the same organ, or the same proximate principle belonging to the plant, may admit at all of this change in its constitution taking place; or if it can, in what degree the presence of this new principle may affect its healthy development.

By turning to the Table which states the relative quantities of alkaline ingredients extracted by water impregnated with carbonic acid from the different soils, it will be seen that in most of these the amount of soda predominated over that of potass, and yet the latter alkali was principally found in their ashes; an indication at least of some superior adaptation of potass to soda with reference to the organization of plants [d].

Again, it is remarkable, that whilst in several of the soils soda appeared to exist in the form of a carbonate (since the quantity of chlorine was so small that only a minute trace of it was discoverable in them), in many of the ashes of the plants only as much soda was detected as would contain sodium equivalent to the chlorine present.

Hence it would seem to follow, that common salt, when it acts beneficially upon land, does not assist the crop by virtue of the alkali it imparts to it, but operates in some other as yet unexplained way; and that it is still questionable, at least in the case of terrestrial species, whether plants have the power of decomposing chloride of sodium, and of separating its chlorine.

Lastly, the analyses contained in this paper may be of use at the present moment, by contributing to shew how much still remains to be done, before we can flatter ourselves at having attained any sure knowledge of the normal constitution of plants, or of the range of variation of which under natural circumstances it is susceptible. At a time when certain enlightened members of the Royal Agricultural Society have prevailed upon that great Body to devote a portion of their funds to the prosecution of the chemical analysis of the ashes of vegetables, whatever tends

[d] This is also shewn very strikingly in a paper on the analysis of Fuci read to the British Association at Cambridge, by Mr. Schweitzer, in June, 1845.

to render more palpable the importance of such an investigation may be of service, in aiding their meritorious efforts to give a more scientific direction to the inquiries which such Associations are intended to promote, and in vindicating the utility of the course which they have in this instance adopted.

Now the facts and observations detailed in the present paper contribute in two respects towards this object, viz., 1st., by shewing that the composition of the most commonly cultivated plants is still open to much uncertainty; and, 2ndly, by pointing out in what way an exact knowledge of their inorganic ingredients might aid us towards the solution of many important practical questions.

I hope it will not be attributed to any blindness on my part to the deficiencies and imperfections which exist in this paper, if I remark, that an investigation of a similar kind to the one herein detailed, if carried out on a more adequate scale, undertaken on ground more carefully selected, conducted with a more vigilant attention to all the minute circumstances which might influence the result, and accompanied by a regular series of analyses, both of the soil and of the crops, during the whole period of their continuance, would be of essential service in clearing up many points in agricultural science which yet remain questionable.

My memoir may serve also as a kind of illustration of that method of scientific book-keeping which I proposed some time ago, at once as an useful exercise for the agricultural student, and as a means of introducing greater precision into the conduct of our experiments on this subject, and which I am therefore happy in having this opportunity of rendering more generally known and understood.

On the Produce obtained from Barley sown in Rocks of Various Ages.

(*From the Quarterly Journal of the Chemical Society, Vol. VII. 1855.*)

IT has been much disputed amongst geologists, whether there be evidences derivable from the structure of the rock-formations which constitute the crust of the globe, such as should lead us to infer, that any of those lying near to the bottom of the series had been deposited at a period antecedent to the commencement of animal and vegetable life.

The affirmative of this proposition is naturally taken by those who contend for the old, and, until lately, universally received opinion, that the great classes into which animals and vegetables are divided appeared upon the earth, not simultaneously, but successively; the invertebral class in the former, and the cryptogamia in the latter, having been first created, in accordance, as theory suggested, with the more chaotic condition of the earlier stages of our planet; whilst the vertebrata and the phanerogamia were brought into being afterwards, when circumstances became more favourable to the existence of plants and animals accomplishing a greater variety of functions, and requiring, therefore, a more elaborate and delicate organization.

Such an opinion must be distinguished from the views of the author of the "Vestiges," as it may be entertained without any participation in the doctrine of transmutation of species; in the one case, it being conceived, that organisms as perfect and as complicated as the conditions of the earth

admitted, were created in every stage of the world's progress, and, therefore, that higher types of the classes which were in existence, often preceded lower ones; in the other, that the superior types, even under possible external conditions, required to be elaborated out of the inferior, and, therefore, necessarily succeeded them.

The negative, however, of both these hypotheses is equally implied in the attempts made by Sir Charles Lyell and his followers[a] to convince us, that the animals which stand highest in the order of being, might all, with the exception of man, have existed at the earliest periods of the earth's creation, no less than those humbler forms of organization of which such abundant vestiges have been preserved; since, if such a position be tenable, there would then be no apparent reason, why the oldest stratified rocks should not have teemed with the remains of animal life as generally, and as copiously, as is found to be the case with respect to those deposited at the present day.

It is not my purpose to detain you with the arguments by which these two theories are respectively maintained; the former supported by the general tenor of observations hitherto made with respect to the organic remains contained in rocks of different ages; the latter fortified by an appeal to certain exceptional cases, in which remains or foot-tracks of animals of higher organization have been found associated with the more characteristic fossils of the stratum, and, consequently, must be inferred to date their existence from a period of greater antiquity than our preconceived notions would have led us to assign to them.

But with regard to the other question, which relates to our recognition of rocks formed antecedently to the existence of organic life, or at least deposited when living beings were just beginning to appear, it cannot, I think, be disputed, but that there is a *primâ facie* case, at least, in favour

[a] This view does not appear to represent the opinions of Sir Charles Lyell at the present time, so far as can be collected from the general tenor of his Remarks in the tenth edition of his "Principles," published in 1863, for he there appears to favour the Darwinian hypothesis, which assumes the evolution of complex forms of life out of simpler ones. (June, 1867.)

of it, in the entire absence of organic remains from many rocks lying below the great Silurian formations, which have been well explored; and also from the gradual dying out, as it were, of the same as we descend in the series[b].

"Thus," as Sir R. Murchison observes, "in Bohemia, as in Great Britain, and in portions of North America, the lowest zone containing fossil remains is underlaid by very thick buttresses of earlier sedimentary accumulations, whether sandstone, schist, or slate, which, though occasionally not more crystalline than the fossiliferous beds above them, have yet afforded no sign of former beings."

The reply to this argument is made by an appeal to our ignorance; by reference to the many cases of metamorphic action that occurred at later periods of the earth's history, from which an entire obliteration of the organic remains of a rock appears to have resulted; and by suggesting, that the chances of metamorphic action having taken place are multiplied in proportion to the antiquity of the formation with which we have to deal.

In considering this question, it occurred to me to ask, whether there were not certain products of the operations of life which might escape destruction, even if all external indications of them were obliterated; and, if so, whether they ought not to remain as permanent monuments of beings that once existed, even after this supposed metamor-

[b] The discovery—which although not altogether undisputed, seems to have obtained general credence—of the fossil called *Eozoon canadense* in the Gneiss of Lower Canada, throws back the commencement of organic life to a period vastly more remote than was supposed at the time when this memoir was published; but it by no means invalidates the conclusion arrived at in it, that the earliest known fossils belonged to the simplest forms of life.

The discovery of the *Eozoon* might also serve to stimulate the inquiry commenced by myself, and suggested in these pages, and induce geologists to examine, whether the rocks in which this supposed fossil has been detected contain any appreciable amount of phosphoric acid; since, from the absence of this ingredient, it might appear a logical consequence, either that they have been premature in assigning an organic origin to the structure which has been supposed to indicate a Rhizopod or Foraminifer, or else, at least, that living beings could not have been numerous at this early stage of their appearance upon the earth. (June, 1867.)

phic action had remodelled the beds in which they were deposited.

It is now many years since[c] I satisfied myself of the existence of phosphoric acid in so many secondary limestones, that I was led at the time to infer that this ingredient existed in them almost universally; and at present its occurrence even amongst metamorphic rocks is generally admitted, not only from the indirect evidence, arising from the fact that plants are able to draw this necessary principle in sufficient quantities from the soil resulting from the decomposition of such rocks, but also by direct analysis, which has established its presence in them.

If this be the case, I see no reason why the earliest rocks with which animal life was associated should not still retain the phosphoric acid which must have formed an integral part of their organization, in which case they would afford, to a certain extent at least, a presumption in favour of the presence of organic remains, before metamorphic action had changed altogether the original structure of the rock.

The occurrence, indeed, of phosphoric acid is no certain criterion of the former existence of animals, because we must assume it to have been present, in common with all the other necessary constituents of organic bodies, before any of the latter came into being; and because, also, we have examples of its appearance, as in the phosphorite rock of Estremadura, in a condition, which compels us to ascribe its deposition to chemical forces alone.

When, for instance, Sir Charles Lyell, in his anniversary address for 1851, p. 36, alleges, that the discovery made by the Government surveyors, of concretions containing between 30 and 40 per cent. of phosphate of lime in the Bala limestone, bears witness to the existence of vertebrata in the most ancient seas, we are inclined to withhold our assent to such a proposition, until assured that this local accumulation of the substance in question could not have been occasioned by those chemical operations, which have worked

[c] Report on Mineral and Thermal Waters: Brit. Assoc. Reports for 1836, p. 20. Three Lectures on Agriculture, 1841, p. 46.

so many changes in rocks, antecedently to, or at least independent of, the agency of living beings. But the converse of the proposition would seem less open to objection, for it is contrary to all analogy to imagine any class of animals to have existed, without leaving behind them some traces of the phosphoric acid, which seems, so far as we know, an universal concomitant of life in all its phases [d].

These traces no doubt become fainter as we descend in the scale of organization, and consequently may, in many instances, escape detection.

The main difficulty, indeed, in the investigation, arises from the minute quantity in which phosphoric acid exists, as compared with the other ingredients of a rock, as well as from the imperfection of our means of separating it, when intermixed, or combined with the other matters present.

I have tried to extract phosphoric acid from the slate of Skiddaw, of Bangor, and of some other localities, operating upon several ounces of each, by long-continued digestion in nitric acid, as recommended to me by Baron Liebig, but could not satisfy myself of the presence of this substance, although I suspected traces of it to exist in the instance of that from Skiddaw. I have, however, no confidence in my power of eliminating, by any chemical reagents at my disposal, from a mass of clay, stone, or loam, a portion of phosphoric acid which does not exceed $\frac{1}{10000}$ part of the entire mass; and yet such a proportion distributed over an acre of ground, which, to a depth of only one foot, contains at least 1,600 tons of earth, would indicate more than 350 lbs. of phos-

[d] Lehmann (Physiological Chemistry) states that phosphate of lime occurs in every kind of animal tissue, in all animal fluids, in all protein compounds more especially. It probably plays an important part in the metamorphosis of the animal tissues, and especially in the formation and in the subsequent changes of animal cells.

Schmidt found, in the inner side of the mouth of Unio and Anodonta, no less than 15 per cent. of phosphate of lime, 3 of carbonate of lime, and 82 of organic matter; so that the phosphate seems to be separated from the blood by this organ, for the purpose of cell-formation. It seems probable that the carbonate is converted in the animal into phosphate by the phosphorus it contains. The latter, however, would seem to arise from the reduction of phosphoric acid. I suspect that phosphoric acid will be found present in all nitrogenized structures, whether of animals or vegetables.

phoric acid,—equivalent to that implied in a very large development of life, belonging to the lower class of animals.

But we may be sure, that if any of those crops, which contain in themselves nutriment for the higher grades of animals, will grow in a particular rock, phosphoric acid in some shape or other must be present in it.

If any doubt exist on that head, I may refer in confirmation of it to the recent experiments of the Prince of Salm-Horstmar [e], which shew, that if the soil be deficient in any of the seven essential elements of plants, of which phosphoric acid is one, no complete development of its parts will take place.

It would seem, therefore, at first sight, as if we had nothing else to do, except to observe whether or not a soil was utterly barren, in order to assure ourselves of the presence of this ingredient; and such undoubtedly would be the case, provided we could ascertain, that neither art nor nature had introduced any foreign body into the materials derived from the subjacent subsoil.

But I know not how this can be confidently predicated, even with reference to a newly-peopled country, much less in the case of one long cultivated, like our own; since it would be necessary to prove, not only that the surface had never been modified by the addition of any kind of manure, but also that no torrent or flood had at any time spread over it materials derived from the country adjacent.

It seemed to me, therefore, that it would be more satisfactory to try the experiment on a small scale, by bringing away from the spots, the subsoil of which it was my wish to test, a sufficient quantity of the pure rock to fill an open box, the dimensions of which should be about two feet square, and of nearly equal depth.

Into this, the material, after having been coarsely pulverized, was introduced; more or less pure sand being added to it, according as this was found necessary, in order to bring the whole into that state of consistency, which appeared likely to render it, mechanically speaking, suitable for vegetation.

[e] *Ann. Ch. Phys.* [3], xxxii. 461, xxxv. 54.

A given weight of barley, in which the amount of ash, and also of phosphoric acid, present had been ascertained by a previous examination, was then sown in each of the boxes. In the experiments made in 1852 and 1853, the quantity of barley sown weighed in each case 120 grains, from which were obtained 3·5 grains of ash, and as nearly as possible 1 grain of phosphoric acid; but finding this quantity unnecessarily large for the earth employed, only 15 grains were introduced in 1854, which, from the results obtained, seemed to be amply sufficient.

Wishing to test in this manner the relative capacity of rocks of different ages to yield phosphoric acid to the crop which grew in them, I selected the following samples, which include most of the rock-formations, from the chalk to the lower Silurian or Cambrian series:—

	Marked.
The chalk from the neighbourhood of Brighton, Sussex,	2
The sand from the calcareous grit of Headington, near Oxford,	3
The oolitic limestone taken from the borders of the Cotswold Hills, near Cirencester, Gloucestershire,	4
The new red sandstone from Haffield, near Ledbury, in Herefordshire,	5
Pure dolomite from the magnesian limestone formation of Roche Abbey, in Derbyshire,	6
Slate taken from a spot near Dolgelly, designated by Professor Sedgwick as a rock lying below the Silurian strata, and apparently destitute of organic remains,	7
Clay slate from the foot of Skiddaw, in Cumberland, also regarded by geologists as lying very low in the series,	8

To these were added, in the present year, two samples of slate from North Wales, both, so far as our examination has yet gone, totally destitute of organic remains, and both lying at the base of the whole series of rock-formations

which have been described by Sir Roderick Murchison and Professor Sedgwick in that country.

	Marked.
These were:—	
The slate from the quarries of Nant Frangon, near Bangor,	9
And that from the quarries of Llanberris, in the same neighbourhood,	10
I also employed a sample of micaceous schist from Loch Lomond,	11

procured through the kindness of Mr. Andrew Liddell, of Glasgow,—a rock in which metamorphic action had entirely obliterated all traces of organic life, if any such were ever present in it.

In order to afford a standard of comparison, there was added to these, in each of the three years, a box containing some of the soil of the Botanic Garden, Oxford (No. 1), where the experiments were carried on, of which an analysis has been already given in my Bakerian Lecture, Phil. Trans. 1845, but which may be described briefly as a made soil of an argillaceous character, resting on the Oxford clay.

The results obtained are given in a connected view in the two tables which accompany this memoir; and I have also constructed a diagram, in which the relative quantities of phosphoric acid extracted from each of the rocks during the several years in which the barley was sown in them are registered [f].

During the years 1852 and 1853, none of the rocks or soils experimented upon were manured, except that from the Botanic Garden; and yet it will be perceived that the crops grown in the former of these years all sufficiently attested the fact, that a portion of phosphoric acid must have been withdrawn from the rock in which they were reared. The proportion, however, varied very considerably; and if we regard the quantity yielded by the crop which was obtained from the Botanic Garden soil, as the normal amount which, under ordinary circumstances, the crop was likely to extract from ground of about the average amount

[f] See the conclusion of this Paper.

of fertility, we shall find that whilst 3·9 grains of phosphoric acid had been absorbed in this instance, no less than 7·73 was extracted from the rich marly sandstone of Ledbury; 2·38 from the chalk; 1·52 from the Headington grit; and quantities varying from 1½ to 1 grain from the remaining rocks.

So far, therefore, there appeared no striking inferiority, on the part of the Cambrian or lower Silurian slates, to the oolite, the Headington grit, or the dolomite of Derbyshire; for the differences in the amount of phosphoric acid taken up by the crop in these several instances were very inconsiderable. But the produce of the year succeeding seemed to indicate, that, in the former cases, the deficiency arose from an actual want of phosphoric acid, whilst in the latter, it was occasioned only by the necessity for a longer period of exposure, in order that the natural agents of decomposition should be enabled to develope, and render soluble, ingredients, which were actually present in the rock, but which, at first, had not been available.

This, I think, may be inferred from the fact, that whereas the oolitic rock in 1853 supplied 4·1 grs. of phosphoric acid, the dolomite 6.53, and the Headington grit 5·55: the slate from Dolgelly, on the other hand, yielded only 0·05, and that from Skiddaw none at all.

In 1854, a similar deficiency in the amount of phosphoric acid obtained, was evinced by the two specimens of Welsh slates which I then operated upon; that from Bangor imparting little more than 0·8 of a grain of phosphoric acid to the barley sown in it, and that from Llanberris none at all; there being, in fact, in this instance, less phosphoric acid in the crop than in the grain sown.

The specimen of micaceous slate, likewise, which I obtained from the neighbourhood of Glasgow, only appeared capable of supplying about 0·6 of a grain of phosphoric acid to the crop which grew in it.

A similar argument, as it appears to me, might be deduced from the fact, that no large amount of lime in any shape at all, as compared with the other ingredients, was discoverable in the slates in question. Thus, after long

digestion in nitric acid, the slate from Dolgelly yielded only 3·2 grs. in the 1,000, or about 0·3 per cent.; the mica schist 1·6 in the 1,000 grs.; the Bangor slate 1·23; the Skiddaw, only a mere trace; the Llanberris slate, none at all. After fusion with carbonate of soda, indeed, these slates, similarly treated, afforded rather a larger quantity of lime, the rock from Dolgelly then indicating as much as 13·2 grs. to the 1,000; the mica schist 4·85 grs.; that from Bangor 4·1 grs.; and from Llanberris 2·2. The Skiddaw slate alone, under neither of these methods of procedure, gave indications of more than a trace of lime. These quantities, however, are all very small in relation to those ordinarily existing in rocks which contain organic remains, unless, indeed, in cases where we may on other grounds suspect a removal of the lime originally present, or a substitution for it of some other ingredient, to have taken place.

Now, as in the case of phosphoric acid, although the presence of lime proves in itself nothing as to the existence of animal or vegetable life cotemporaneously with the deposition of the rock, its absence would seem to indicate the want, and its deficiency the paucity at least, of lime-secreting beings.

It remains to be seen, whether rocks of equal antiquity contain lime in such a condition—that is, so disseminated through the rock—as to lead to the contrary inference; for its occurrence in veins, or in alternating strata, would prove nothing, inasmuch as chemical forces alone might have been capable of bringing it there, and of producing the arrangement which it exhibits.

To return, however, to the case of the phosphoric acid, it might be said, that the failure in these instances arose, not from the want of this ingredient, but from some mechanical unsuitableness in the rock for rearing a crop of the above description; but this objection was obviated by adding, in 1854, to the same sample of Dolgelly and of Skiddaw slates which had been made the subject of experiment in the two preceding years, a portion of phosphate of lime and of nitrate of soda.

The same trial was also made upon the two Welsh slates

obtained in 1854, and in these instances in a manner even more conclusive, because the manured and unmanured rock was experimented upon at the very same time.

Now the great increase of crop obtained in all the four instances by manuring, sufficiently evinces that there was no mechanical impediment offered by any of those samples of rock to the growth of barley; the following being the results obtained:—

In Dolgelly slate, the phosphoric acid extracted from the rock in 1853 was only 0·05; in 1854, the rock being manured, 1·06.

Skiddaw slate .	in 1853,	unmanured	0·00
" " .	" 1854,	manured .	1·93
Slate from Bangor	" "	unmanured	0·08
" " .	" "	manured .	1·71
" from Llanberris	" "	unmanured	0·00
" " .	" "	manured .	1·00 grs.

The same inference may probably be drawn from the fact, that in those rocks which evinced a deficiency in phosphoric acid, the proportion of straw to grain exceeded the average, so that it may be concluded that the barley made an effort to develope itself, and proceeded in its growth, until it was arrested by the want of phosphoric acid [g].

As this ingredient is not uniformly distributed through all the parts of a growing plant, but is found principally in the grain, and even in this is confined to the gluten, starch being almost destitute of it [h], it seemed to me desirable not to content myself, in every instance, with merely calculating the probable amount of phosphoric acid from that of the ash obtained, but in some cases to determine it by actual experiment; and accordingly it will be found, by reference to the table, that in the case of the Bangor, Skiddaw, Llanberris, and Dolgelly slates, less phosphoric acid was detected in the ashes than is normally present in the same amount of barley.

[g] See the experiments recorded in the *Annales de Chimie* for 1851, by the Prince of Salm-Horstmar.

[h] See my Bakerian Lecture, pp. 239, 240.

It must be admitted that the above experiments, even though conducted upon a sound principle, do not go the length of substantiating the entire absence of phosphoric acid in any of the cases examined, except in that of the Llanberris slate, where, as will be seen, there was even at first less phosphoric acid in the crop than in the grain previously sown, whilst in the others I always observed in the first year a small increase, amounting in the case of the Bangor slate to about $\frac{8}{10}$ of a grain; in that of the Dolgelly rock, to about $1\frac{1}{2}$ grs.; in that of the Skiddaw slate to $1{\cdot}2$; and in the micaceous rock from Loch Lomond, to about $\frac{6}{10}$ of a grain.

Nevertheless, the marked diminution which, in these cases, took place in the second year, serves to shew, that the first crop had really taken up nearly all which the rock was able to afford; and in these cases the inference would seem to be, that we had approached the borders, at least, of the lowest limit of organic existence; for, as the whole quantity of rock contained in each of the boxes exceeded 60 lbs. or 460,800 grains, we should have no right to calculate upon more than from about $\frac{1}{300000}$ part of phosphoric acid in the rock which yielded the largest amount of this ingredient, and little more than $\frac{1}{1000000}$ in that which supplied the smallest.

Now in the case of the Cambrian slate from Dolgelly, the stock of phosphoric acid appeared the second year to be *nearly*, and in that from Skiddaw, *wholly* exhausted, although, by the addition of phosphate of lime to each, they were brought back, as will be seen, to more than their original fertility.

If, then, we compare the above proportions of phosphoric acid with those ascertained by the most recent investigations to exist in secondary limestones, or in soils derived from them, we shall find an enormous disproportion between the two; few of the latter that have been analyzed appearing to contain less than one part of phosphoric acid in 10,000, and many probably as much as one part in the 1,000.

If it be objected, that the phosphoric acid might be locked up in the rock in such a manner as to prevent its being

given out to the growing crop, it may be replied that we have no right to infer the existence of a body which has neither been detected by direct chemical analysis, nor shewn to be present by the indirect method resorted to, until at least more refined researches shall either have determined its presence, or have pointed out the manner in which, having been once deposited in the rock, it may have been dissipated by the metamorphic action which subsequently took place.

It may also be objected, that my experiments are too limited in number and extent to warrant any general conclusions being deduced from them; nor, indeed, have I the presumption to suppose, that they alone are capable of settling so large a question as that which has been mooted in this paper.

I may, however, remark, that whilst in all the secondary rocks I examined, the usual proportion of phosphoric acid was discoverable by my method, a marked deficiency in this ingredient was perceived in every one of the samples taken from the bottom of the Silurian or of the Cambrian series which I had collected.

It may, moreover, be pointed out, that these samples were chosen, not from one locality, but from a number of places, for the most part quite remote one from the other, in England, Scotland, and Wales,—and that, in the case of the Bangor slate, the rock employed consisted of the powder rubbed off from a great variety of slates, taken from all parts of that extensive quarry, by the grinding or sawing process to which they are subjected in order to bring them to an uniform size; and that they may therefore be presumed to present a fair average of the rock in general.

Indeed, when we recollect that not a trace of any organic body has been detected by the most careful examination, notwithstanding the vast quantities of this slate that have been exposed to observation within the last century, in consequence of the enormous demand that has arisen for the material, both at home and abroad, it is evident that, if organic remains were ever present, the most complete metamorphic action must have operated throughout the mass,

in order to bring about so total an obliteration of them; nor could such a process have taken place without diffusing the phosphates throughout the rock, so as to afford traces of it everywhere.

At any rate, I hope to have succeeded in opening a line of inquiry, which, if further pursued, may lead to the future elucidation of this problem, and, at the same time, may have an immediate practical bearing upon agriculture itself, by pointing out which of the rock-formations are most likely to be deficient in phosphoric acid, and, therefore, most susceptible of amelioration by manures containing that principle.

June, 1867.

In answer to some inquiries made to Mr. M. Salter, F.G.S., relative to this question, I received from him the following information, which, although not affecting the conclusion I had arrived at, with respect to the slates of Bangor, Dolgelly, Llanberris, Skiddaw, and Glasgow, shews, that traces of organic life are recognisable in earlier rocks elsewhere:—

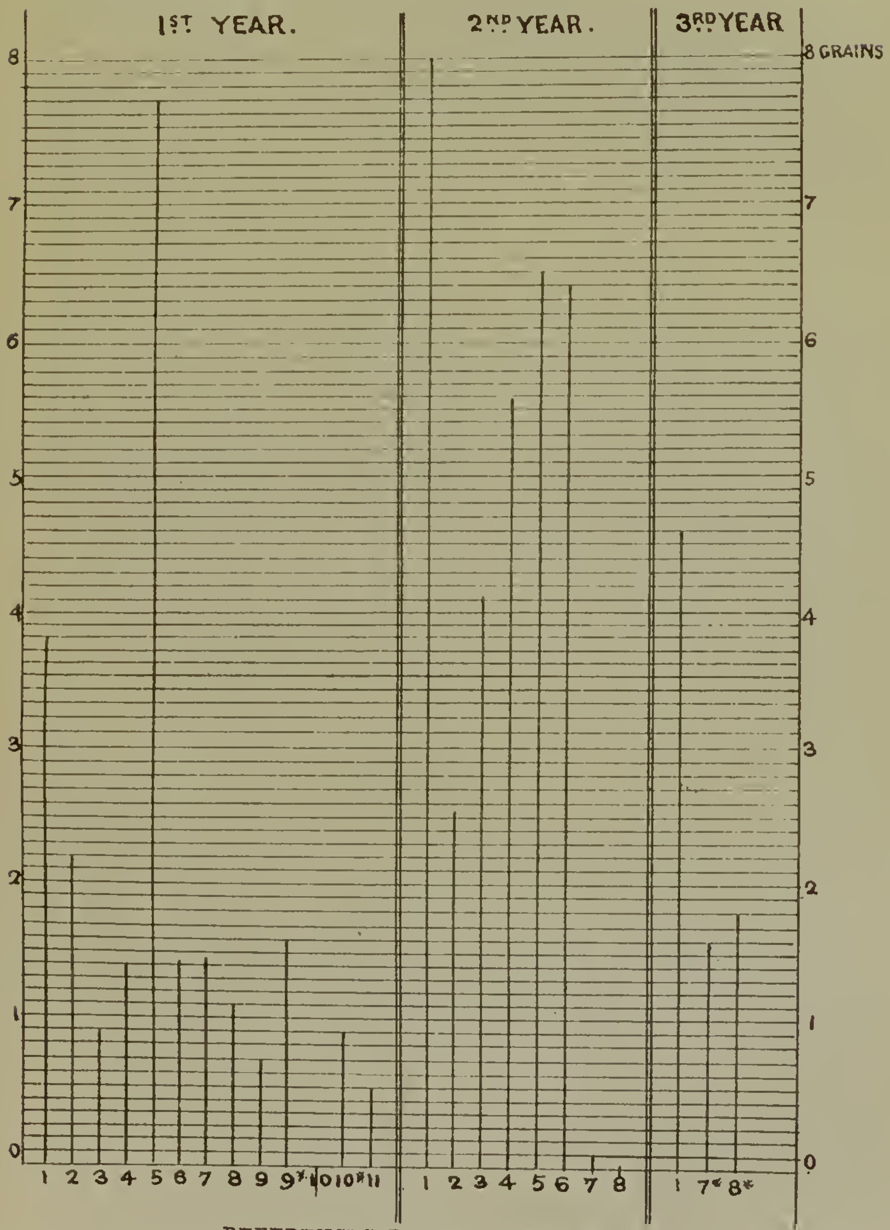
"The Lower Cambrian (purple slates and sandstones, with chloritic slate and grey sandstone) is rich in organic life in some spots, at others, apparently quite destitute of it. The plants (*Nullipores* or *Acetabulariæ*) which are known by the name of *Oldhamia*, are found both in reddish and grey schists of this age in Wicklow. And with them multitudes of tubes of worms which have perforated the beds in every way, chiefly in a vertical direction, as the common shore worm (*arenicola*) of our coasts does now. In 1856 I first discovered these worm-tubes throughout a mile or more of thickness, in the red and purple slates of the Longmynd, Shropshire; and with them what I still believe to be the head and caudal shield of a Crustacean, whether Trilobite or Phyllopod I will not say. A thin bed of carbonate of lime occurred below these fossils.

"In 1857 I found large worm-tubes at Moel-y-ci, near Bangor (Mem. Geol. Survey, vol. iii. p. 213, fig. 1); and similar fossils were met with by Dr. A. Fritsch, in Bohemia, in beds of like age.

"Still more lately, a shell, *Lingucella*, has been discovered by my friend and coadjutor, Dr. Henry Hicks, of St. David's, Pembrokeshire, in the purple grits near that place. So that we may regard the Lower Cambrian no longer as an azoic rock, but one stored with those lower forms of Articulata and Mollusca, which, preceded as they must have been by sea plants, are, so far as we know, the earliest known forms of life on our globe, for the nature of *Eozoon* is, to say the least, problematical, and, in my opinion, it is only a mineral."

TABLE,

Shewing the relation between the amount of phosphoric acid present in the barley sown, and in the crop obtained from it, in the several soils below-mentioned :—



REFERENCES TO THE NUMBERS.

1. Botanic Garden Soil.
2. Chalk of Brighton.
3. Oolite, Cirencester.
4. Sand, Headington.
5. Sandstone, Ledbury.
6. Dolomite, Roche Abbey.
7. Clay Slate, Dolgelly.
7*. Ditto, manured.
8. Clay Slate. Skiddaw.
8*. Ditto, manured.
9. Clay Slate, Bangor.
9*. Ditto, manured.
10. Clay Slate, Llanberris.
10*. Ditto, manured.
11. Mica Schist, Glasgow.

Rock in which the Barley was sown.	Produce from the Barley sown in 1852, 1853, and 1854.			Ash obtained from the Crops in 1852, 1853, and 1854.			
1. Soil of the Botanic Garden, Oxford, being made ground resting on the Oxford Clay:—	Manured. 1852.	1853.	1854.	Manured. 1852.		Manured. 1853.	1854.
Grain	428	591	565	. . . .	9·5	17·0	16·00
Straw	1132	1730	1695	. . . .	127·0	178·0	100·00
Total	1560	2321	2260		136·5	195·0	116·00
Weight of Barley sown . . .	120	120	15	Weight of Ash in Barley sown	3·5	3·5	·44
Gain	1446	2201	2245	Gain . .	133	191·5	115·56
	Unmanured. 1852.	1853.		1852.		1853.	
2. Chalk, Brighton:—							
Grain	327·0	267·0		. . . .	7·0	. . . . 8·0	
Straw	1233·0	630·0		. . . .	77·0	. . . . 54·5	
Total	1560.0	897·0			84·0	62·5	
Weight of Barley sown . . .	120·0	120·0		Weight of Ash in Barley sown	3·5	. . . . 3·5	
Gain	1440·0	777·0		Gain . .	80·5	Gain . 59·0	
3. Oolitic Limestone, Cotswold Hills, near Cirencester:—							
Grain	142·0	370·0		. . . .	3·5	. . . . 13·0	
Straw	698·0	920·0		. . . .	54·0	. . . . 79·0	
Total	840·0	1290·0			57·5	92·0	
Weight of Barley sown . . .	120·0	120·0		Weight of Ash in Barley sown	3·5	. . . . 3·5	
Gain	720·0	1170·0		Gain . .	54·0	Gain . 89·5	
4. Sand, from Headington, Oxfordshire:—							
Grain	175·0	396·0		. . . .	5·0	. . . . 12·0	
Straw	665·0	1680·0		. . . .	62·0	. . . . 130·0	
Total	840·0	2076·0			67·0	142·0	
Weight of Barley sown . . .	120·0	120·0		Weight of Ash in Barley sown	3·5	. . . . 3·5	
Gain	720·0	1956·0		Gain . .	63·5	Gain . 139·5	
5. Red Sandstone, Ledbury, Herefordshire:—							
Grain	769·0	410·0		. . . .	20·5	. . . . 15·5	
Straw	1991·0	1250·0		. . . .	159·0	. . . . 104·1	
Total	2760·0	1660·0			179 5	119·6	
Weight of Barley sown . . .	120·0	120·0		Weight of Ash in Barley sown	3·5	. . . . 3·5	
Gain	2610·0	1540·0		Gain . .	176 0	Gain . 116·1	
6. Magnesian Limestone, Roebe Abbey, Derbyshire:—							
Grain	227·0	496·0		. . . .	5·5	. . . . 17·3	
Straw	793·0	1470·0		. . . .	54·0	. . . . 143 0	
Total	1020·0	1966·0			59·5	160·3	
Weight of Barley sown . . .	120·0	120·0		Weight of Ash in Barley sown	3·5	. . . . 3·5	
Gain	900·0	1846·0		Gain . .	56 0	Gain . 156·8	

Amount of Phosphoric Acid present in the Crop of 1852, 1853, and 1854.						Relation between the Organic and Inorganic Matter existing in the Crop obtained in 1852, 1853, and 1854.			Relation between the two Crops, the Ratio being in 1852, 1853, and 1854,		
Unmanured. 1852.		Unmanured. 1853.		Manured. 1854.		1852.	1853.	1854.	1852.	1853.	1854.
experiment	2·6	By experiment	5·9	By experiment	2·840	Grain 100	100·0	100·	Grain 100	137	151
calculation	2·3	By calculation	3·2	By calculation	1·800	Ash . 2·88	2·88	2·83	Ash . 100	179	169
Total . .	4·9	Total . .	9·1	Total .	4·640	Straw 100	100·0	100·	Straw 100	153	150
osp. Acid esent in rley sown .	1·0	. . . .	1·0	. . . . . .	·125	Ash 10·30	10·30	5·90	Ash . 100	140	78·5
Gain . .	3·9	Gain . . .	8·1	Gain . .	4·515						

1852.		1853.		1852.	1853.	1852.	1853.
y calculation . . .	2·00	By experiment . . .	2·50	Grain as . 100	100	Grain as 100	81·0
y experiment . . .	1·38	By calculation . . .	0·97	Ash . . 2·11	3·00	Ash as 100	114·0
	3·38		3·47	Straw as . 100	100	Straw as 100	51·5
hosphor'c Acid in Barley sown .	1·00	. . . .	1·00	Ash . . 6·35	8·6	Ash as 100	70·5
Gain	2·38	Gain	2 47				
y calculation . . .	1·00	By calculation . . .	3·7	Grain as . 100	100	Grain as 100	264
y calculation . . .	0·97	By calculation . . .	1·4	Ash . . 2·4	3·5	Ash as 100	371
	1·97		5·1	Straw as . 100	100	Straw as 100	132
hosphoric Acid in Barley sown .	1·00	. . . .	1·0	Ash . . 7·7	8·6	Ash as 100	146
Gain	·97	Gain	4·1				
y calculation . . .	1·42	By experiment . . .	4·20	Grain as . 100	100	Grain as 100	225·0
y calculation . . .	1·19	By calculation . . .	2·35	Ash . . 2·85	3·02	Ash as 100	240·0
	2·52		6·55	Straw as . 100	100	Straw as 100	254·0
hosphoric Acid in Barley sown .	1·00	. . . .	1·00	Ash . . 9·40	7·55	Ash as 100	210·0
Gain	1·52	Gain	5·55				
y calculation . . .	5·85	By experiment . . .	5·60	Grain as . 100·0	100·00	Grain as 100	53·5
y calculation . . .	2·88	By calculation . . .	1·80	Ash . . 2·68	3·78	Ash as 100	75·0
	8·73		7·40	Straw as . 100·0	100·00	Straw as 100	62·5
hosphoric Acid in Barley sown .	1·00	. . . .	1·00	Ash . . 8·00	8·40	Ash as 100	65·0
Gain	7·73	Gain	6·40				
y experiment . . .	1·50	By calculation . . .	4·95	Grain as 100·00	100·00	Grain as 100	213·0
y calculation . . .	0·97	By calculation . . .	2·58	Ash . . 2·42	3·46	Ash as 100	314·0
	2·47		7·53	Straw as . 100·0	100·00	Straw as 100	186·0
hosphoric Acid in Barley sown .	1·00	. . . .	1·00	Ash . . 6·85	9·80	Ash as 100	265·0
Gain	1·47	Gain	6·53				

Rock in which the Barley was sown.	Produce from the Barley sown in 1852, 1853, and 1854.			Ash obtained from the Crops in 1852, 1853, and 1854.			
	Unmanured.		Manured.	Unmanured.		Unmanured.	Manured.
7. Cambrian Slate, near Dolgelly :—	1852.	1853.	1854.	1852.		1853.	1854.
Grain	184·0	138·0	330·0	. . . .	5·0	2 2	9·50
Straw	836·0	155·0	448·0	. . . .	54·5	17·7	26·00
Total	1020·0	293·0	778·0		59·5	19·9	35·50
Weight of Barley sown . . .	120·0	120·0	15·0	Weight of Ash in Barley sown	3·5	3·5	·44
Gain	900·0	173·0	763·0	Gain . .	56·0	16·4	35·06
8. Clay Slate, from Skiddaw, Cumberland :—							
Grain	290·0	127·5	350·0	. . . .	7·6	2·8	10·10
Straw	910 0	150·0	537·0	. . . .	68·0	14·8	34·10
Total	1200·0	277·5	887·0		75·6	17·6	44·20
Weight of Barley sown . . .	120·0	120·0	15·0	Weight of Ash in Barley sown	3·5	3·5	·44
Gain	1080·0	157·5	872·0	Gain . .	72·1	14·1	43·76
	1854.			1854.			
	Unmanured.	Manured.		Unmanured.			Manured.
9. Bangor Slate :—							
Grain	75·0	330·0		. . . .	2·20		7·00
Straw	294·0	1222·0		. . . .	18·00		71·00
Total	369·0	1552·0			20·20		78·00
Weight of Barley sown . . .	15·0	15·0		Weight of Ash in Barley sown . .	0·44		·44
Gain	354·0	1537·0			19·76		67·66
10. Llanberris Slate :—							
Grain	4·0	164·0		. . . .	0·20		4·70
Straw	27·0	466·0		. . . .	2·30		27·80
Total	31·0	630·0			2·50		32 50
Weight of Barley sown . . .	15·0	15 0		Weight of Ash in Barley sown . .	·44		·44
Gain	16·0	615·0			2·06		32·06
	Unmanured. 1854.			1854.			
11. Micaceous Slate, near Glasgow :—							
Grain	60 0				1 30		
Straw	342·0				28 30		
Total	402·0				29·60		
Weight of Barley sown . . .	15·0			Weight of Ash in Barley sown	·44		
Gain	387 0			Gain . . .	29·16		

Amount of Phosphoric Acid present in the Crop of 1852, 1853, and 1854.

Unmanured. 1852.		Unmanured. 1853.		Manured. 1854.	
By experiment	1·500	By experiment .	0·55	By experiment	1·260
By calculation	0·970	By experiment .	0·50	By calculation	0·470
Total .	2·470	Total . .	1·05	Total . .	1·730
Phos. Acid present in Barley sown	1·00	. . . . . .	1·00	. . . .	·125
Gain . .	1·47	Gain . .	0·05	Gain . .	1·605

Unmanured. 1852.		Unmanured. 1853.		Manured. 1854.	
By experiment	1·0	By experiment .	0·25	By experiment	1·400
By calculation	1·2	By calculation .	0·26	By calculation	0 660
Total .	2·2	Total . .	0·51	Total . .	2·060
Phos. Acid present in Barley sown	1·0	Loss . .	0·49	. . . .	·125
Gain . .	1·2	Phos. Acid present in Barley sown	1·00	Gain . .	1 935

1854.

Unmanured.		Manured.	
By calculation . . .	0.640	By experiment . . .	0·560
By calculation . . .	0·322	By calculation . . .	1·280
	0·962		1·840
Phosphoric Acid in Barley sown .	0·125	. . . .	·125
Gain	0·837	Gain	1·715

Unmanured.		Manured.	
By calculation . . .	0·062	By calculation . . .	0·652
By calculation . . .	0·041	By calculation . . .	0·500
	0·103		1·152
Loss	·22	Phosphoric Acid in Barley sown .	·125
Phosphoric Acid in Barley sown .	0·125	Gain	1·027

1854.

By calculation	0·182
By calculation	0 510
	·692
Phosphoric Acid in Barley sown	·125
Gain	·567

Relation between the Organic and Inorganic Matter existing in the Crop obtained in 1852, 1853, and 1854.

	Unmanured. 1852.	Unmanured. 1853.	Manured. 1854.
Grain	100	100	100·
Ash .	2·70	1·60	2 87
Straw	100	100	100·
Ash .	6·50	11·4	5·80
Grain	100	100·0	100·
Ash .	2·62	2·2	2·88
Straw	100	100·0	100·
Ash .	7·50	9 9	6·30

1854.

	Unmanured.		Manured.
Grain .	100	Grain	100
Ash .	2·9	Ash	2·1
Straw .	100	Straw	100
Ash .	6 1	Ash	5·8
Grain .	100	Grain	100
Ash .	5·0	Ash	2·85
Straw .	100	Straw	100
Ash .	8·5	Ash	6·5

1854.

Grain as . .	100·00
Ash	2·16
Straw as . .	100·00
Ash	6·80

Relation between the two Crops, the Ratio being in 1852, 1853, and 1854.

	Unmanured. 1852.	Unmanured. 1853.	Manured. 1854.
Grain	100	72·5	180·0
Ash .	100	75	180
Straw	100	18·0	53·0
Ash .	100	18·5	53·5
Grain	100	44	121
Ash .	100	37	133
Straw	100	16·5	59
Ash .	100	21·6	50

1854.

	Unmanured.	Manured.
Grain .	100	440
Ash . .	100	317
Straw .	100	410
Ash . .	100	394
Grain .	100	4100
Ash . .	100	2350
Grain .	100	1730
Ash . .	100	1200

On the occurrence of Fluorine in Recent as well as in Fossil Bones.

(*From the Journal of the Chemical Society for 1845.*)

HAVING, in the course of the preceding spring, paid a visit to the deposit of compact phosphorite which occurs in the province of Estremadura in Spain, I was subsequently led to examine into the chemical constitution of the mineral, which forms the prevailing ingredient of the mass to which my inquiries had been directed.

The results of this examination have already been reported in the Memoir, communicated by my fellow-traveller Captain Widdrington, R.N., and myself to the Geological Society with respect to the rock in question, and read at their meeting on the 17th of January last; from which it will be seen, that although the material, from not being crystallized, is somewhat variable in its composition, yet when selected as pure as possible, it contains as much as 81 per cent. of phosphate of lime, and 14 per cent. of fluoride of calcium, the remainder appearing to consist of silica and peroxide of iron [a].

[a] The phosphate of lime I have since satisfied myself to possess the same composition as apatite, viz. $PO_5 + 3CaO$; and this was confirmed by Mr. Middleton and Mr. D. Campbell, in the laboratory of Professor Graham, who were kind enough each to undertake an independent analysis of the mineral. They likewise agree with me, and with each other, in their estimation of the amount of fluoride of calcium, determined by the loss occasioned on the addition of sulphuric acid to the pounded specimen, from which loss the proportion of fluorine was calculated. I should, however, remark that this quantity is double that of the largest amount reported by Rose to be present in the crystallized apatites which he examined (see Thomson's Mineralogy, vol. i. p. 125); and that I have never succeeded in obtaining more than 8·8 grains per cent. of fluoride of calcium by the direct method, in which the fluorine was expelled by heat and sulphuric acid from the phosphorite contained in a platina still, and where, after passing it through a silver vessel kept at 500°, in which any earthy matter carried over with the vapour might be deposited, it was finally received into

The conclusion arrived at with respect to the compact form of mineral phosphate of lime occurring in the above locality, coupled with the reports of other chemists to the same fact relative to crystallized apatite, naturally led me to speculate as to the final causes of the apparently constant association of fluoride of calcium with earthy phosphates

a glass vessel containing caustic ammonia. As sulphate and fluate of ammonia could alone be formed, I estimated the amount of fluorine by adding chloride of calcium, and separating the resulting sulphate of lime from the fluoride of calcium by repeated washings with water. I apprehend, however, that in this method the whole of the fluorine may not have been driven over, in spite of all the pains bestowed upon the process.

I am desirous of appending the analyses given by Messrs. Middleton and Campbell of the mineral, as, though they differ, as might be expected, somewhat from my own, in the entire quantity of phosphate of lime present in the specimens they examined, they confirm, nevertheless, my statement as to the relation both between the phosphoric acid and the lime, and between the phosphate of lime and fluoride of calcium present. The portion examined by Mr. Middleton was in the form of a brown earth, containing nodules of a pure white, corresponding probably with the original condition of the formation generally. The results of analysis were as follow :—

Phosphate of lime	73·66
Fluoride of calcium	15·10
Phosphate of iron	4·26
Peroxide of iron	1·16
Matter insoluble in acids	3·95
Loss on heating to low redness after thorough drying	·78
Loss in the process	1·09
	100·00

The constitution of the phosphate seems to be $CaF + 2\ (3CaO\ PO_5)$.

Analysis by Mr. Campbell.

1. 6·89 grains of a mixed substance, as received from Dr. Daubeny :—

Phosphate of lime ($3CaO\ PO_5$)	4·78 =	69·38
Phosphate of iron	·70 =	10·16
Fluoride of calcium	1·01 =	14·66
Peroxide of iron	·02 =	·29
Insoluble matter in acids	·38 =	5·51
	6·89 =	100·00

2. Analysis of white crystalline solid mass; 11·00 grains gave,—

Phosphate of lime ($3CaO\ PO_5$)	8·68 =	78·90
Fluoride of calcium	·63 =	14·82
Matter insoluble in acids	·66 =	6·00
Loss in experiment	·03 =	·28
	11·00 =	100·00

amongst the older materials of the globe, and to ask myself, whether it might not be possible, that fluorine, as well as phosphorus, fulfilled some hitherto unexplained office, in the economy of those organic beings, for the sake of which such mineral matters may be conjectured to have been treasured up in the rock formations from the beginning of time.

These reflections brought to my mind the researches of Morichini and of Berzelius with respect to the existence of fluorine in bones, seeing that the latter, according to the concurrent testimony of both these philosophers, appear to contain as a constant ingredient a minute quantity of fluoride of calcium, inasmuch as its presence is vouched for by them, in recent as well as in fossil bones, and in the teeth of mammalia, as well as in other parts of their osseous structure.

Here, however, I was compelled to pause, by observing the contrary statements put forth by other able chemists relative to this point; Fourcroy and Vauquelin having, previously to the researches of Berzelius, denied the existence of fluorine in recent bones, and Dr. Rees having, subsequently to them, in a memoir drawn up under a full knowledge of what had been done before, arrived at a conclusion equally opposed to that of the Swedish philosopher; one, too, which has been since corroborated, in a communication relative to the composition of bones made to the French Institute, by Messrs. Girardin and Preisser of Rouen, and lately published in the *Comptes Rendus*.

As, however, none of these gentlemen appear to dispute that fluorine does occur *in fossil bones* generally, the conclusion they arrived at leaves the subject, it must be confessed, encumbered with greater difficulties than before; for as all sound chemical analogies stand opposed to the admission of the idea, that fluorine can have been generated from the other constituents, during any process of decay or alteration that might have occurred in it, during the ages that had elapsed since it formed a part of the living structure, we should be driven to the belief, that the fluoride of calcium contained in bone had filtered in from without,—a conjec-

ture which, although perfectly plausible, if the occurrence of this ingredient had been casual, or had been limited to bones found in rocks of a certain age or composition, seemed rather a violent one, when extended to those of all ages and formations, being scarcely reconcileable with the rarity of the mineral itself in the waters of springs, and with its sparing solubility in most re-agents.

These difficulties that occurred to my mind, no less than the weight I attached to the positive testimony of the great Swedish chemist in favour of the existence of fluorine in recent bones, induced me to consider, whether it might not be possible that certain circumstances had operated in the mode of conducting the experiment, by which the presence of fluorine in the hands of the chemists who adopt the opposite conclusion escaped detection.

And on further investigation it appeared to me probable that two ingredients naturally present in recent bones might have interfered with the result in the instances alluded to.

The first of these is animal matter, which, owing to the strong affinity it possesses for fluorine, may arrest its escape, and thus prevent it from coming into contact with the glass.

The second might be the presence of salts containing some volatilizable ingredient, such as the carbonic or muriatic acids, liable to be disengaged by the same agent by which the fluorine was set at liberty, and, when escaping in a rapid current, likely to carry along with them the latter, before it had time to exert any sensible action upon the glass suspended over it. Accordingly I found, that whilst one-tenth of a grain of fluor spar mixed with more than 100 grains of any earthy mineral, occasioned under the action of sulphuric acid an easily discernible, though faint corrosion on the exposed parts of the glass, the same quantity produced no effect whatever, when mixed with 5 per cent. of carbonate of lime, or with a little gelatine; and that half a grain of fluor spar and the earth, when mixed with gelatine, caused a trace on glass not much more distinct than that occasioned by one-tenth of a grain without this admixture.

In testing, therefore, the bones and teeth which I had obtained for examination, I did not content myself with merely adding sulphuric acid to the pulverized specimen, but I began by burning off all the animal matter; and then, finding that the carbonic acid still in part remained, I dissolved the earthy residuum in muriatic acid, and threw down by means of caustic ammonia the earthy phosphates.

The latter, after being well washed and dried, were treated with concentrated sulphuric acid in a platina crucible, covered over by a plate of glass, shielded, except on the parts intended to be acted upon, by a coating of wax; but no artificial heat was applied, as the sulphuric acid, by its action upon the phosphate, raised the temperature sufficiently to expel whatever fluoric acid might be present in the specimen.

The glass was allowed to remain as a cover to the platina crucible for at least two hours, and in order to ensure the condensation upon it of the hydrofluoric vapour, a rim of wax was placed round the margin of its upper surface, by means of which a small portion of water might be kept the whole time in contact with it, so as to maintain a suitably low temperature.

That these precautions were not unnecessary I satisfied myself, by observing the difference in the degree of corrosion produced by a fossil bone given me by Dr. Buckland from the cave of Kirkdale in Yorkshire, when thus purified from the animal matter of which its long interment had not yet deprived it, as well as of its carbonic acid, as compared with the same, when treated with sulphuric acid without having undergone such a preparation.

In proof of this I submit to the inspection of members specimens No. 3 and No. 4; the one shewing the glass corroded by a Kirkdale bone deprived of its animal matter and carbonic acid; the latter, by one retaining both. Operating in this manner, I have succeeded in engraving upon glass, not only by means of fossil bones, from Stonesfield, from Montmartre, from the cave of Kirkdale in Yorkshire, and from that of Gailenreuth in Franconia, specimens of all

which were supplied me by Dr. Buckland; but likewise with the bone of some quadruped that had been lying for a long, but unknown time, exposed to the weather in the soil of our Botanic Garden; with the vertebra of an ox recently killed; with the tibia of a human subject from an anatomical cabinet at Oxford; with the teeth of an ox just killed; and with human teeth of recent date. The markings differ widely in the degree of their distinctness, and are in some instances so faint as hardly to be discerned except by daylight; but I have convinced myself that they cannot be attributed to the disengagement of phosphoric acid, as the same glass was in no degree affected by the fumes proceeding from the action of sulphuric acid upon pure phosphate of lime, where the acid had been derived from the direct combustion of phosphorus, nor, for a long time at least, by the vapour of free phosphorie acid exposed to a heat sufficiently great to fuse and partially to volatilize it.

Nor was it dependent on any peculiarity in the nature of the glass, for plate glass was corroded in the same manner as the crown glass more usually employed.

By the oldest and most fossilized specimens the glass seemed undoubtedly to be most deeply etched; yet even here there occurred exceptions, for the marks caused by a bear's bone taken from Gailenreuth are the faintest in the whole series, and were produced only after a long exposure to the acid vapours, two previous trials having proved unsuccessful; whilst, on the other hand, the tibia of a human subject gave indications almost as distinct as any even of the fossil bones operated upon.

It would have been more satisfactory if I could have stated the proportion of fluorine in these samples of bones and teeth, as well as the fact of its actual presence, and likewise if I had extended my examination over a larger number of specimens; but I have been compelled to postpone the former part of the inquiry until I could obtain an apparatus suitable for the purpose, and felt doubtful when my time would permit me to carry further the present investigation, if, in order to give my results in a state of greater completeness, I neglected the present opportunity of com-

municating them[b]. The only criterion, therefore, I am at present enabled to offer as to the proportion of fluorine in the bones examined, is a comparison of the depth and distinctness of the marks produced by the latter, with those caused by a certain known amount of fluor spar, mixed with a weight of phosphate of lime or other earthy material equal to that present in the bones operated upon. Judging by this rough mode of measurement, it would appear, that in several instances the faintness of the mark shews a smaller quantity of fluorine to have been present in the specimen, than would have been contained in a mixture of one-tenth of a grain of fluor spar added to 100 grains of phosphate of lime.

The existence of fluoric acid, as a constant, or at least a common ingredient in bones of all ages, would seem *à priori* to be much more probable, than its absence in recent bones would be, if its normal presence in fossil ones be admitted, for we can readily understand its finding its way into the animal structure through the medium of plants, which may imbibe it along with those phosphates with which it is so generally associated. Indeed it seems so likely, that those vegetables at least that contain much phosphate of lime should possess a trace of it, that I am at this very time examining the ashes of barley with reference to the latter point[c].

The greater distinctness of the marks produced by the fossil bones acted upon, than by the recent ones, may be more difficult of explanation; but before it is urged as an objection against the view taken, it should be deter-

[b] I have since, by the aid of the apparatus described in the former note, attempted to estimate the amount of fluorine in the fossil bone from Stonesfield, and in the recent human bone from an anatomical cabinet. The former afforded 8·7 grains per cent. of fluoride of calcium, the latter only 2·0, results which will at least indicate the *relative*, if not the *absolute* quantity of fluorine present.

[c] I have since ascertained that no sensible action is exerted on glass by heating with sulphuric acid the earthy phosphates present in 12 lbs. of barley. Sprengel, I find, had already suggested the probable occurrence of fluorine in plants, but conceives that it exists in such a state of combination, as causes it to be dissipated by the heat necessary for expelling the carbonaceous matter, and therefore cannot be detected in the ordinary method.

mined, whether the difference may not arise from the removal of the greater part of the animal matter from the fossil bone, owing to its long interment in the earth. Of the six specimens of fossil and recent bones of which I made a rough analysis, that from Stonesfield, which was the oldest of any, having been embedded in a secondary rock belonging to the oolite formation, lost, by exposure to a heat of 212°, 4·2 per cent.; by a further heat of about 500°, 5·0 per cent. more, and by increasing the temperature to a red heat only 1·8 per cent. in addition, the latter probably representing very nearly the amount of animal matter remaining, the two former numbers the water retained within the bone.

Proceeding upon the same data, the bone from the tertiary rocks of the Paris basin, next in the order of antiquity, would contain 10 per cent. of water, 2 of animal matter. The bone from Gailenreuth, water 13·9, animal matter 5·0; that from Kirkdale 12·5 water, 11 animal matter; whereas the recent bone picked up in the Botanic Garden contained, even when dry externally, about 30 per cent. of water and 11 animal matter; and the human tibia, which had been kept in an anatomical cabinet for a certain time, gave out 23 per cent. of water and 17 of animal matter.

It may also be suggested, as a possible explanation, that the fluoride of calcium distributed through the mass had in the course of time become collected into little nuclei in certain parts of the bone, and for this reason might allow of a more ready disengagement from it of the fluoric acid which it contains as an ingredient.

That a certain alteration in the arrangement of the earthy particles of a bone does occasionally take place after its deposition, is evidenced from the curious observations by Messrs. Girardin and Preisser, in the memoir which has been already referred to, as these gentlemen state, that the bone-earth phosphate appears in some instances to have separated into two distinct compounds, crystals of apatite being recognised by them in some of the fossil bones in their possession, which they conceive to have arisen from the segregation of the tribasic from the bibasic compound.

Will not this latter fact also help us towards an under-

standing of the function which fluate of lime may fulfil in the structure of bones, and likewise of the peculiar adaptation of the bone-earth phosphate to serve as its prevailing earthy ingredient?

It seems a general law in both kingdoms of organic nature, that crystallization should operate as a sort of antagonistic force to the processes of assimilation, so that no material can be fitted to enter into the fabric of a living body, between whose particles the natural force of polarity operates with all its energy. Hence, according to Dr. Prout, the use of the infinitesimal portions of foreign inorganic matter interposed between the particles of most bodies which form the constituents of vegetable or animal organization; and although it may be true, as has been suggested by Von Buch, that the very prismatic form which belongs to phosphate of lime as a mineral species, adapts it for the fibrous structure of bone better than other earthy compounds in which the axis of crystallization is equal in both directions, yet even in this case the tendency to arrange itself according to the laws which regulate inert matter might operate too powerfully, were it not diminished by the association in equal atomic weights of the two phosphates, each of which possesses a polarity in some degree differing from the other, and consequently to a certain extent counteracts the disposition in the particles of the other to assume a determinate arrangement.

If there be any truth in these speculations, is it not also conceivable, that the interposition of a mineral matter, like fluor spar, whose particles crystallize in quite another manner, that is, in cubes, may co-operate on the same principle, in imparting that freedom of motion to the particles of the prevailing constituent of bones, by which it is rendered more pliant to the purposes of the animal economy, more obedient to the laws of life, more ready in short to insinuate itself into the pores, so as to form the coats of those delicate capillary canals of which the osseous structure appears to consist?

Memoir on the Occurrence of Iodine and Bromine in certain Mineral Waters of South Britain.

(From the Philosophical Transactions for 1830.)

THE discovery in sea-water of iodine and bromine, two principles which, although in minute proportions, are said to be generally diffused throughout the present ocean, naturally suggested the inquiry, as to whether these same ingredients might not be found to exist in springs occurring in inland situations when containing a similar saline impregnation. This accordingly has been already determined by Stromeyer, Liebig, and others, to be the case in many of the brine-springs of Germany, France, and Italy; but at the time my attention was first directed to the subject, I was unacquainted with any trials of the kind having been instituted with reference to those of this country, except by Professor Turner, of the London University, regarding the presence of iodine in the mineral waters of Scotland; in only one of which, that of Bonnigton, near Leith, he appears to have detected it. I was therefore induced, in the course of last spring and summer, to undertake a pretty extensive survey of such English springs as are known to contain a considerable proportion of common salt; and having succeeded in detecting in several of them traces of one or both the substances alluded to, I inserted a brief account of the results obtained in the "Philosophical Magazine and Annals of Philosophy" for September last.

An article that has appeared in a subsequent number of the same periodical work has, however, been the means of drawing my attention to a little work by Mr. Murray, entitled "Experiments on Chemical Philosophy," which had not before fallen in my way; and from this it is clear, that the detection of iodine in the Gloucester Spa water had been made by that gentleman some time before I had engaged in the inquiry. I am unable, however, to discover in his

publication, although it bears so late a date as 1828, anything that can substantiate the assertion which its author has made in the number of the "Philosophical Magazine and Annals" referred to, as to his having anticipated me in the discovery of iodine in the springs at Cheltenham [a], or in that of bromine in those of Ingestrie. I consider myself, therefore, still warranted in claiming as my own the first public announcement of the existence of bromine in our English springs; but I am far from attaching importance to a discovery which had been previously made in so many similar situations abroad, and would wish it to be understood, that my only pretence for offering to the Royal Society the present communication, is the circumstance of my having examined on the spot most of the mineral springs hereafter enumerated, and having undertaken, wherever it appeared practicable, to obtain an approximation at least to the proportion which these principles bore to the other ingredients present, and to estimate their comparative frequency and abundance in the several rock-formations.

To the geologist, the results of such an inquiry may be of interest, as tending to identify the products of the ancient seas in their most minute particulars with those of the present ocean: and to the physician it may be an object of curiosity, to speculate how far the unexplained virtues attributed to certain mineral waters depend on the presence of these ingredients, the energy of whose action may perhaps compensate for the minute quantity in which they are found. I confess, indeed, that with regard to the former of them, iodine, we ought to be sceptical as to any medicinal agency that can be exerted by so small a quantity as a single grain diffused through ten gallons of water, the largest proportion in which I have ever detected it. But with respect to the second, bromine, after considering the statement of its discoverer, M. Balard, as to its highly poisonous operation upon animals, which my own experience of the irritating

[a] Mr. Ainsworth, however, one of the editors of the "Edinburgh Journal of Natural and Geographical Science," states that he had communicated the fact of the existence of iodine in the Cheltenham waters previously to my announcement of it.

effects of its vapour tends fully to confirm, I cannot view it as absurd to trace the medical virtues ascribed to such waters as those of Ashby-de-la-Zouch, to the presence of even so small a quantity as a grain of hydro-bromate of magnesia (if such be the combination) in each pint of the water; and that the proportion would not fall far short of that, my experiments on this particular spring seem to warrant me in concluding. It is curious, at least, that almost the only two brine-springs, properly so called, which have acquired any reputation as medicinal agents, that of Kreutznach, in the Palatinate, and that of Ashby-de-la-Zouch, in Leicestershire, both should contain a larger proportion than common of this new principle; and that in either instance that reputation should have been enjoyed, long before any suspicion as to their peculiar nature could have been entertained.

The objects I had in view in this inquiry being what are above stated, I have chosen to classify the springs noticed in the accompanying Table according to the geological position of the strata from which they issue; and under the head of each have set down the total amount of their saline ingredients; the nature and proportion of them as ascertained by former chemists, or, whenever I could not depend upon the results, by myself; and the proportion which the iodine and bromine, where either of these principles existed, bore to the quantity of water, and likewise to that of the chlorine which the solid ingredients of the spring might contain. The latter statement has been introduced in order to remove an impression which may have been created in consequence of the detection of iodine, as it is said, even in common pump-water [b], when very large quantities of it were evaporated; from which circumstance it might be inferred, that this principle is not only a constant accompaniment of common salt, but that its quantity bears a pretty regular ratio to that of the latter ingredient. Although I have myself evaporated no less than forty-eight gallons of the Oxford pump-water without finding the slightest trace of iodine in the last por-

[b] Mr. Cuff, a chemist at Bath, has also detected it in the hot springs of that place, by evaporating about thirty gallons of the water.

tions, I shall not dispute the truth of the former position, which might possibly have been borne out, had still larger quantities been operated upon [c]; but that the latter opinion is untenable, will be readily seen from the accompanying Table, which shews that the proportion of iodine to chlorine varies in every possible degree, and that the springs most strongly impregnated with common salt are in some instances those in which I have evaporated the largest quantity without detecting any trace of iodine. The same remark will equally apply to bromine; so that the general inference seems to be, that although these two principles may perhaps be never entirely absent where the muriates occur, yet that their distribution is certainly very unequal, and therefore forms a proper subject of scientific research. The quantity in which the former of these ingredients occurs in mineral waters is commonly so inconsiderable, that I have been unable to determine it by direct analysis, and have been therefore obliged to content myself with obtaining an approximation to its real amount. In the case of one of the Leamington springs, indeed, I employed the agency of nitrate of silver to precipitate the iodine from the concentrated water, and afterwards separated by means of ammonia the chloride from the iodide of silver obtained. I have reason, however, to believe, from some comparative experiments, that where the proportion which this latter ingredient bears to the former one is extremely small, it may be taken up either wholly or in part by ammonia; and I therefore contented myself in other instances with evaporating the water until it began to produce the characteristic blue or violet tinge with starch and sulphuric acid. This was then compared with the colour imparted by the same test to a solution of hydriodate of potass of known strength; and the latter, if not of the same shade already, was brought to it by dilution with a measured quan-

[c] I am also loth to question the fact (stated on good authority) of the existence of a minute proportion of iodine in sea-water, although I have reduced ten gallons of it, taken from the English Channel, near Cowes, to less than half-an-ounce, without being able to detect any in the residuum. There seems reason, however, to infer, from what is stated in the next page, that the starch-test cannot be relied upon to detect very minute quantities of iodine, when a comparatively large proportion of bromine is present in the same solution.

tity of water. Having thus noted the proportion of iodine in the test liquor with which the concentrated solution corresponded, it was easy to calculate what it must have been in the mineral water itself, by knowing the number of times its original quantity had been reduced by evaporation previously to the employment of this re-agent.

The sulphates and muriates present in brine-springs do not appear to interfere with the delicacy of this test; but where bromine was also present, I have seen the liquor, either at the time or shortly after the operation of the reagent, assume a pinkish hue, owing, as I suppose, to the reddish tinge of the bromine given out mixing with the blue colour of the iodide of starch. In stating, therefore, as I have done in the Table, the proportions of iodine in several of the waters, I am far from pretending to offer more than an approximation to the relative quantity in which it occurs, and am fully aware of the necessity of more precise experiments, conducted on a different principle, before the points in question can be considered as satisfactorily determined.

The starch test I find will readily indicate a quantity of iodine not exceeding one grain to seven gallons of water, or $\frac{1}{450000}$ part; but as in no case that has occurred to me, the proportion exceeded one grain to ten or twelve gallons, and in many appeared scarcely to amount to $\frac{1}{10}$ of that quantity, I despaired of arriving at more accurate results, by adopting any other method that aspired to greater precision than the one already stated.

In every case in which I have noted that no iodine could be detected, the water had been concentrated at least as far as to $\frac{1}{30}$ of its original quantity without effect; so that the proportion of this principle, supposing after all any of it to exist, could not well amount to a grain in 200 gallons. In some cases indeed, where the spring was one of weak impregnation, I have carried the concentration much further, as may be seen in the Llandrindod waters, where no traces of iodine appeared, until they had been reduced to nearly $\frac{1}{60}$ of their original volume.

In my trials for bromine, I have in great measure conformed to the directions of Balard; first boiling down the

water to about a fourth of its original quantity with a portion of quicklime to prevent the bromine from being dissipated by the heat; and then, after filtering the residuum, introducing chlorine as long as any sensible yellowness was caused by its addition. The water was then strongly agitated with ether, which collects on the surface, carrying with it the bromine with which it had combined, and was then poured off into a separate vessel. [The bromine, immediately upon being thus removed from the water, was treated with a quantity of a concentrated solution of pure soda sufficient to render the ether containing it colourless; the latter alkali being employed for this purpose in preference to the vegetable one, as I found that bromine formed with sodium a salt more soluble in alcohol than it did with potassium.]

Unfortunately, however, the salts which are contained in or deposited from the ethereal solution after the addition of the soda, appear to be of a very mixed description, consisting not only of the hydrobromate and bromate of soda, but also of the muriate and chlorate, together with a little uncombined alkali, if the proportion of the sodium to the bromine is not very nicely adjusted. I therefore began by heating the whole product sufficiently to convert the bromate of soda into the bromide, and the chlorate into the chloride, of sodium; and afterwards, in order to ensure the union of any alkali which may have been in excess with carbonic acid, I dissolved the whole in water impregnated with that gas. The solution was then brought to dryness, and strong alcohol added to separate the bromide of sodium as much as possible from the other ingredients; after which, the alcoholic solution, having been evaporated, was re-dissolved in water, and nitrate of silver added to it in order to form the insoluble bromide of silver, the weight of which when dried and melted, would determine that of the bromine present, every 100 grains, according to M. Balard, indicating 41.1 of this principle.

From the weight of the precipitate, however, I felt myself obliged to make a large deduction, in proportion to the quantity of alcohol employed, for the chloride of sodium at the same time taken up; having ascertained by a previous

experiment how much common salt a given quantity of this menstruum could dissolve. The latter part of the process, however, being liable to some uncertainty, I should have preferred, had my engagements permitted, re-examining the waters on the spot, and operating on such quantities of them as would have enabled me to extract appreciable quantities of bromine. This, indeed, I have done in the case of the Middlewich water, but not with sufficient attention to the quantities employed and obtained, to enable me to calculate in this manner the exact proportions between them: with regard to the other springs, the quantity of water which I could conveniently transport to my laboratory was not such as to enable me to pursue with much hope of success this particular method. It is therefore with diffidence that I offer provisionally the statements given in the Table, as an approximation to the relative quantities of bromine existing in some of our English springs, calculated according to the scheme of analysis above stated; and shall hope at some future and not very distant period to obtain results more worthy of reliance, should my further labours on this subject not be rendered in the meantime unnecessary by the investigations of some other chemist. In cases where the quantity of this principle appeared to be less considerable, as in the Leamington, Cheltenham, and Gloucester waters, I have contented myself with guessing at its proportion by concentrating the water until it assumed a decidedly yellow tinge with chlorine, noting what proportion of bromine in water produced a colour of equal intensity.

The earliest of the rock formations in this country that come under our consideration with reference to the present inquiry, is the greywacke slate of North Wales[d], which in the neighbourhood of Builth, in Radnorshire, gives out springs containing a notable proportion of common salt. Those of Llandrindod have long enjoyed some reputation as medicinal agents, but their composition does not appear to have been correctly ascertained; for the most modern analysis I have seen[e] assigns to them a considerable proportion of

[d] Lower Silurian Rocks of Murchison: see "Silurian System," p. 325.

[e] Analysis of the Llandrindod Waters, by Mr. Williams, surgeon, 1819.

muriate of magnesia, of which I find scarcely a trace. The more newly-discovered springs at Builth itself, though less celebrated, are similar in point of constitution; and being double the strength of those of Llandrindod, ought to possess superior medical virtues. At both places are waters which differ from the rest in containing an unimportant impregnation of sulphuretted hydrogen, but in other respects correspond.

Many of our coal-pits emit streams of salt water; but the most remarkable stream of the kind is that already noticed, of Ashby-de-la-Zouch, in Leicestershire, which for the last few years, especially since the erection of the baths, which are now so great an ornament to the spot, has acquired a certain local reputation in the cure of diseases. Previously to the discovery of bromine, of which I detected in this water an appreciable quantity, Dr. Thomson, of Glasgow, had examined its composition; and I have therefore been satisfied with adhering to the results of his analysis, which is stated in the Table.

The most important, however, of the salt springs that we meet with in this country are those in the new red sandstone formation of Cheshire; for an analysis of which I may refer to a paper of Dr. Henry's, published in the "Transactions" of this Society. In this instance, also, I have adopted the statements of another, merely making a proportionate deduction from the amount of the ingredients given by that able chemist, in consequence of the weaker impregnation of the samples of water I employed, than of those on which he appears to have operated.

It will be seen by reference to the Table, that all the brine-springs of that district contain bromine, and most of them iodine; indeed it is probable that if I had had time to concentrate larger quantities of the water, the latter would have been detected throughout. It may be remarked, however, that the rock-salt of Northwich, in Cheshire, contains no trace of either principle; a circumstance explicable from the more deliquescent nature of the hydriodic and hydrobromic salts, which would cause them, together with the earthy

muriates, to remain in the mother-liquor after the common salt had crystallized, and thus to become distributed through the substance of the marly beds afterwards formed over the rock-salt, from which the brine-springs appear to derive their saline impregnation.

There is a blue variety of rock-salt met with at Ischel, near Saltzburg, which, from the resemblance between its colouring matter and that of the compound of starch and iodine, might be suspected to contain this latter principle united with some kind of vegetable matter. I have been unable to obtain a specimen deeply enough tinged with the colouring matter alluded to, to set the question completely at rest; but on dissolving a portion of the blue salt, which I obtained through the kindness of Mr. Heuland, in water, not the slightest tinge appeared to be communicated to the solution, neither did any blueness appear on recrystallizing the salt. The specimen alluded to gave no indications of iodine when tested with starch in the usual manner, and was nearly pure from admixture with foreign ingredients, although it appeared to contain a trace of sulphuric acid and of lime. At present, therefore, I am inclined to attribute the colour rather to some peculiar arrangement of the particles of the common salt itself, than to the presence of any other ingredient.

The springs containing purgative salts, which arise from the lias clay in various places along its whole range from Leamington to Gloucester, appear to be derived from the same source as the brine-springs of Cheshire and Worcestershire above alluded to; but their saline contents have been modified by the sulphuric acid generated by the decomposition of the sulphuret of iron present in the stratum from which they immediately proceed. Hence the proportion of earthy muriates is usually greater in them than in the brine-springs properly so called; because the muriatic acid disengaged by the action of the sulphuric acid upon the common salt has dissolved a fresh quantity of lime or magnesia from the surrounding materials of the rock.

If such be the origin of the sulphates of soda and magnesia which impart to these waters their aperient quality,

it would be natural to expect that they should be found in greater abundance on the first discovery of the spring than after it has been long drawn upon; and hence, perhaps, the remarkable discrepancy between the results of my examination of the Gloucester and some other waters, and those given on the authority of former chemists, may be explained, without impeaching the accuracy of either.

It will be seen by reference to the Table, that I have represented the ingredients of the Leamington waters on the authority of Dr. Thomson, as stated by Dr. Loudon in his "Practical Dissertation[e]" on these springs; and those of Cheltenham, with the exception of one lately discovered at Pittville, on that of Dr. Scudamore. The springs of Tewkesbury and Gloucester I have myself examined; there being of the former no analysis at all, and of the latter only one by Mr. Accum, which I had reason to believe, what I in fact found, quite inapplicable to its present composition. The spring which goes by the name of the Chalybeate Saline is at present destitute of iron, which I am assured it formerly possessed, whilst the Sulphureous contains no trace of sulphuretted hydrogen. These two springs, which at present appear almost identical, are the ones most strongly impregnated with purgative salts, and therefore approximate more nearly to the character of that analyzed by Mr. Accum, according to his representation, than either of those termed "the pure saline," which he professed to have examined. Many of these springs, it will be seen, contain traces of bromine and iodine; but they seem to be less common in the aperient waters which are occasionally met with in the chalk and tertiary districts of this country; for I have examined three—those, namely, of Epsom, of Chad's Well, in Gray's-Inn-lane, and of St. Leonard's Hill, near Windsor, without discovering traces of iodine in any one. In the Epsom water alone a slight trace of bromine was perceptible.

With regard to the state of combination in which these principles occur, I have only to observe, that they are no doubt combined with hydrogen, forming the hydriodic and hydrobromic acids, and neutralized in all probability by

[e] Leamington, 1828.

magnesia, both forming with this basis salts decomposable at a low temperature, which seems to be the case with the compounds of both bromine and iodine existing in the waters I have examined. Even long-continued boiling, there is reason to believe, diminishes the quantity of bromine originally present; and hence it seems advisable, when the object is to estimate the whole of this principle which a mineral water may contain, to combine the hydrobromic acid with lime, in the manner which I have recommended to be done when speaking of the mode of separating bromine from its combination.

I may conclude by observing, that there is little question as to the possibility of procuring a sufficient supply of bromine from our English brine-springs, should a demand be created for this new substance, either for medical purposes or for the arts of life; for, from a few rough trials of its comparative abundance in the Middlewich and Ashby springs, and in those of Kreutznach, in the Palatinate, which affords, it is said, the principal supply for present consumption, I should regard our own quite as highly charged: neither can it be doubted but that the proportion of bromine present in many brine-springs exceeds considerably that contained in the present ocean, which, from experiments recently made by myself on water taken from the English Channel a short distance from Cowes, I have stated in the Table as existing in the proportion of one grain to the gallon.

Table comprehending a List of certain Springs in South Britain, which contain
tion of this and the other Ingredients

Geological Position.	Locality of the Spring.	Name of the Spring.	Total of its Saline Contents in the Pint.	Iodine. Its Proportion to The Water.	The Chlorine.
Transition Slates.	Llandrindod, Radnorshire.	No. 1. The pure Saline.	31·35 Gr.	Seems not to exceed 1 Grain to 343 Gallons.	As 1 to 50·000
	Ditto.	No. 2. The Chalybeate Saline.	Nearly as No. 1.	None detected.	. . .
	Builth, Radnorshire.	No. 1. The Saline.	77·6 Gr.	A trace, nearly the same as that exhibited by No. 1. Llandrindod.	As 1 to 120·000
	Ditto.	No. 2. Sulphuretted Saline.	Nearly as No. 1.	None detected.	. . .
	Ditto.	No. 3. The Chalybeate Saline.	Nearly as No. 1.	None detected.	. . .
Coal Formation.	Ashby-de-la-Zouch, Leicestershire.	The Moira Brine Spring.	179·88 Gr.	None detected.	. . .
	Near Newcastle-upon-Tyne.	The Walker Colliery Brine Spring.	192·0 Gr.	None detected.	. . .
	Kingswood, near Bristol.	The Soundwell Colliery Brine Spring.	64·0 Gr.	None detected.	. . .
	Northwich, Cheshire.	Brine Spring.	1696 Gr.	None detected.	. . .
	Middlewich, Cheshire.	Ditto.	1824 Gr.	A trace, probably not exceeding 1 Grain to 543 Gallons.	As 1 to 2·650·000.
	Nantwich, Cheshire.	Ditto.	1760 Gr.	1 Grain to about 12 Gallons.	As 1 to 96 000
	Wheelock, Cheshire.	Ditto.	1440 Gr.	A trace, apparently not greater than that in the Middlewich.	Nearly as 1 to 2·000·000
	Droitwich, Worcestershire.	Ditto	1746 Gr.	None detected.	. . .
	Shirleywich, Staffordshire.	Ditto.	1552 Gr.	None detected.	. . .
Lias Clay.	Leamington, Warwickshire.	No. 1. Robbins's Well.	95·948 Gr.	1 Grain to about 10 Gallons.	As 1 to 3440
	Ditto.	No. 2. Royal Pump Saline Spring.	134·749	Rather less than in No. 1.	. . .
	Ditto.	No. 3. Smith's Pump.	109·992	A trace, but apparently not more than a Grain to 192 Gallons.	As 1 to about 48 000.
	Ditto.	No. 4. Wise's Pump.	107·396	Nearly as the preceding.	. . .
	Ditto.	No. 5. Smart's Saline.	92·589	Nearly as the two preceding.	. . .
	Ditto.	No. 6. Lord Aylesford's.	113·995	None detected.	. . .
	Ditto.	No. 7. Reid's Sulphureous.	79 142	None detected.	. . .
	Ditto*.	No. 8. Reid's Saline.	102·597	None detected.	. . .
	Gloucester.	No. 1. Sulphureous Saline.	84·2	About 1 Grain to 50 Gallons.	As 1 to 12·000
	Ditto.	No. 2. Chalybeate Saline.	In all respects agrees with No. 1 in point of composition, and		
	Ditto.	No. 3. Strong Saline.	76·5	About 1 Grain to 96 Gallons.	As 1 to 33·000
	Ditto.	No. 4. Weak Saline.	75·22	Nearly as No. 3.	. . .
	Tewkesbury.	The Walton Spring.	46·1	About 1 Grain to 36 Gallons.	As 1 to 6690
	Cheltenham.	Pittville, No. 1. "The Pure Saline."	45·8	None detected.	. . .
	Ditto.	Sherborne, No. 4.	84·44	About 1 Grain to 90 Gallons.	As 1 to 33·000
	Ditto.	Thomson's, No. 4.	80·13	About 1 Grain to 30 Gallons.	As 1 to 3600
	Ditto.	Old Well, No. 1.	81·51	About 1 Grain to 60 Gallons.	As 1 to 19·000
	Ditto*.	Thomson's, No. 2.	52·29	None detected.	. . .
Oolitic Strata.	Melksham, Wilts.	The Saline Spring.	107·42	None detected.	. . .
Chalk Formation.	Epsom, Surrey.	The Saline Spring.	33·2	None detected.	. . .
Tertiary Rocks.	Windsor.	St. Leonard's Hill Spring.		None detected.	. . .
	Gray's-Inn-lane, London.	Chad's Well.		None detected.	. . .
Present Ocean.	Off Portsmouth.			None detected.	. . .

* In none of the remaining Leamington or Cheltenham Springs

common Salt in considerable quantity: together with a Statement of the Propor-

present in a Pint of each.

Bromine. Its Proportion to The Water.	The Chlorine.	Chlorides of Calcium.	Magnesium.	Sodium.	Sulphates of Lime.	Magnesia.	Soda.	Peroxide of Iron.	Carbonates or other Salts.	Authority on which the Statement of the Saline Ingredients is given.
Not estimated, but a distinct trace.	Not estimated.	6·6	24·75							Daubeny.
As No. 1.	. . .	Ingre	dients as	No. 1.	except	that it	contai	ns 0·6		Daubeny.
A trace, not estimated.	. . .	11·2	A trace	66·4						Daubeny.
A trace, not estimated.	. . .	Ingre	dients	nearly	as No.	1				Daubeny.
A trace, not estimated.	. . .	Ingre	dients as	No. 1.	except	that it	contai	ns a trace		Daubeny.
1 Gallon seems to contain 4·68 Grains.	As 1 to 180	36·4	3·72	133·0	4·24		2·52			Thomson.
A trace, not estimated.	. . .	2·8	2·8	186·0						Daubeny.
None detected.	. . .	2·5		58·5			3·0			Daubeny.
1 Gallon seems to contain 1·2 Grains.	As 1 to 6600	0·42	1·27	1667·0	25·5				Insoluble matter 1·696	Henry.
9·36 Grains of Brome in 1 Gallon.	As 1 to 860	0·5	1·37	1793	27·35				Insoluble matter 1·3	Henry.
6·82 Grains of Brome in 1 Gallon.	As 1 to 1275	0·45	1·32	1730	26·5				Insoluble matter 1·76	Henry.
A trace, not estimated.	. . .	0·36	1·1	1115	22·0				Insoluble matter 1·44	Henry.
None detected.	. . .		A trace	1691			40·25			Daubeny.
4·32 Grains of Brome to 1 Gallon.	As 1 to 1720	38·0	22·0	1190						Daubeny.
1 Grain to about 10 Quarts.	As 1 to 430	23·5	8·468	35·35			28·619	A trace		Thomson.
Nearly as strong as No. 1.	. . .	20·902	12·365	67·78			32·744	0·956		Thomson.
A trace.	. . .	19·772	2·121	47·865			40·234	A trace		Thomson.
A trace.	. . .	18·777	22·592	26·61			39·457	A trace		Thomson.
A trace.	. . .	17·570	26·05	14·534			34·435	A trace		Thomson.
A trace.	. . .	20·561	3·266	40·77			40·398	A trace		Thomson.
A trace.	. . .	15·777	9·695	25·60			28·065	A trace		Thomson.
A trace.	. . .	17·987	10·813	42·92			30·61	0·265		Thomson.
About 1 Grain to 10 Quarts.	As 1 to 600			50·41	1·2		10·35		Carbonate of Lime 0·2	Daubeny.
very nearly in the pr	oportion of its	ingred	ients.							Daubeny.
Rather less than in No. 1.	As 1 to 860			71·5	2·0		1·6			Daubeny.
Nearly as No. 3.	. . .			69·2	2·38		1·15			Daubeny.
None detected.	. . .	0·3	1·8	37·5			5·6		Carbonate of Lime 1·0	Daubeny.
About 1 Grain to 6 Gallons.	As 1 to 768		A trace	27·16			17·55		Carbonate of Lime 0·2	Daubeny.
None detected.	. . .	4·29	0·59	72·8			6·76			Scudamore.
None detected.	. . .	3·07	2·02	46·4			28·64			Scudamore.
None detected.	. . .	6·21	2·54	58·2			14·56			Scudamore.
A trace, about as much as in the Spring at Pittville.	. . .	3·31	1·52	25·7			21·76			Scudamore.
A trace.	. . .	12·0	0·42	90·0			3·9			Daubeny.
A trace.	. . .			6·0	18·8				Carbonate of Lime 5·0	Daubeny.
None detected.	. . .									
None detected.	. . .									
1 Grain to 1 Gallon.	As 1 to 840									

could I satisfy myself of the existence of either iodine or bromine.

PART II.

GEOLOGICAL MEMOIRS.

Some Account of the Eruption of Vesuvius which occurred in the Month of August, 1834, Extracted from the Manuscript Notes of the Cavaliere Monticelli, Foreign Member of the Geological Society, and from other Sources; together with a Statement of the Products of the Eruption, and of the Condition of the Volcano subsequently to it.

(*From the Phil. Trans., vol. 125, for 1835.*)

THE eruption of Vesuvius which occurred in the month of August of last year, excited on the spot an unusual share of interest, from the largeness of the volume of lava at the time discharged, and the extent of the damage it occasioned in its progress down the mountain; whilst, in a scientific point of view, it attracted the greater attention, since it was regarded by many as the concluding link in a series of volcanic operations which had been going on up to that period, with only occasional intermissions, from the year 1831.

It was therefore natural, that on my arrival at Naples shortly after the mountain had subsided into a state of comparative repose, I should seize upon the opportunity which appeared to offer of increasing my acquaintance with volcanic phenomena; first, by collecting on the spot such information as could be best relied on, with respect to the leading features of the past eruption; and secondly, by ascertaining from personal examination the actual condition of the volcano, and the products resulting either from its late operations, or from those in actual progress.

With a view to the former object I solicited and obtained from the Cavaliere Monticelli (one of the Foreign Members of the Geological Society) a written account of the eruption,

from which he has permitted me to extract such particulars as I might deem likely to interest the Members of the Royal Society; whilst in the hope of accomplishing the latter object, a considerable portion of the time I spent at Naples was taken up in visiting the several parts of Vesuvius and in collecting the solid as well as aëriform substances ejected from its crater, and from the recently erupted lava.

In the former part, therefore, of the present communication, I can claim no further share than as the compiler of facts observed and reported to me by others; and all that I conceive myself personally responsible for is the latter portion, in which I have stated the several products and actual condition of the volcano at the time I visited it.

It would appear that for a considerable time previous to the eruption in question, the crater of the volcano had continued to throw up stones and scoriæ, which falling down, for the most part, almost perpendicularly round the point of their emission, had by degrees accumulated into two conical masses, which rose up in the midst of the great crater. The largest of these cones is calculated to have been more than 200 ft. in height, and possessed at one time a regular pyramidal form, with an appearance of stability.

It is stated, however, by Monticelli, that in May last, from the 20th of which month up to the 20th of July the volcano had continued to throw up stones and ashes, and even to emit lava, both these conical hillocks were observed to be broken away, and to sink towards the south; whence, in a memoir read by him to the Academy of Sciences at Naples on the 5th of August, he predicted their speedy disappearance.

These anticipations were realized at no long period subsequently. On the 22nd of August, after the volcano had continued for a month in a state of apparent repose, volumes of black smoke began to shew themselves on the summit of the more recent of the two hillocks above noticed; and after a smart shock of an earthquake, this was succeeded by ejections of red-hot stones and scoriæ, which continued to be shot forth all the night, with fresh quakings and rumblings of the soil.

Early on the 23rd, a current of lava was seen to issue from the foot of the great cone which encompasses the crater on its western side, and this, bending in the direction of the point called Crocelle, reached the flanks of the rising ground denominated Contaroni, whence, moving continually forwards at the rate of about six feet per minute, and reinforced by a second stream of lava which had burst forth from an adjacent point, it reached about nightfall the path generally taken from the Hermitage to the summit of the mountain, which it completely blocked up.

During the 24th, lava continued to flow from the same points, and to advance down the western declivity of the mountain; and during the night a violent shaking of the volcano, which agitated the whole adjacent country, was apparently coincident with the falling in of both the conical hillocks described as existing in the interior of the crater, no traces of which were visible in the morning. Thus we have here a decided instance of two considerable pyramidal masses of volcanic materials, not blown into the air, as some might suppose to be the case, but actually swallowed up within the cavities of the mountain in the course of a single night.

Up to this time the western side of the volcano had been the point that yielded to the internal pressure, and the inhabitants of Portici and Resina had imagined themselves to be chiefly menaced. But on the evening of the 24th a fresh vent was established on the eastern side of the mountain near the *Grotta del Mauro*, whence the lava of 1817 had issued; and after this had taken place, no more lava was observed to flow from the western side of the cone. On the other hand, the current from the eastern side was reinforced on the morning of the 25th by a second stream, which, issuing forth from the foot of the great cone on the spot called Coutrel, flowed over the preceding one.

On the morning of the 26th, an immense column of black and dense smoke served as the prelude to the bursting forth of a new current of lava from the same point as before, as well as from several others in the neighbourhood; and the whole of this molten mass poured down the mountain in

a single narrow stream, circumscribed within the boundaries of a hollow way or water-course. Here, its progress being favoured by the rapid slope of the declivity, it very soon reached Mauro, and took possession of the road leading from Bosco-tre-case to Ottayano.

On the 27th it was augmented by two fresh currents emitted from points not far distant; but now, instead of flowing on in a single stream as before, it became divided into three. The largest of these currents, going straight in the direction of Mauro, spread over some lands belonging to the hamlet of Toreigno; the second covered the cultivated fields above Bosco-reale; and the third invaded the upper part of the village of Bosco-tre-case.

It was the first, however, of these currents which effected the greatest damage. Widening as it descended, it had acquired, by the time it reached the base of the mountain, a breadth of nearly half a mile, retaining even there a depth which averaged from fifteen to eighteen feet.

At Mauro, the Casino of the Prince of Ottayano formed its precise boundary to the north, and one wall of that mansion was swept away by it, whilst all the rest of the building stood uninjured. From this point the lava proceeded to the road which leads from Torre del Annunziata to Ottayano, which it completely blocked up, and moving still further to the eastward, swept away in its course several detached hamlets included in the Commune.

It is calculated, that 180 houses, the abodes of about 800 persons, were destroyed by the current, and that 500 acres (moggie) of land were covered over and reduced to sterility by it.

Among the remains of the houses overthrown by the lava which I was able to examine, no traces of fusion were visible, and the lava seemed to have acted merely as so much dead weight pressing upon them from without. These, however, it is to be remarked, were on the verge of the stream where the lava was least hot, for in the interior of the current I was unable to discover any vestiges of the houses that had been destroyed.

At the time that the eruption occurred, the villages in the

neighbourhood were covered to the depth, it is said, of two inches by a shower of capilli; and from one account which I have seen, it would appear that torrents of hot water were poured down from the crater on the 28th.

The flow of lava from the crater continued all the 29th; but subsequently to that date no further eruption was perceived, and the principal current already described, being no longer urged forwards or augmented by fresh streams from above, gradually slackened in its progress, and stopped at a distance of about a quarter of a mile beyond the road from Torre to Ottayano.

The lava is said to have been accompanied throughout its progress by a cloud of black sand which hovered over its path, and from this cloud emanated frequent flashings of very vivid lightning, sometimes, but not always, followed by thunder.

These flashings Monticelli refers to the particles of sand being in an opposite state of electricity to that of the air, and consequently, when diffused through it by the wind, producing a discharge of electrical light. The same phenomenon was remarked by him in the preceding month of May, at which time the volcano, as has been stated, emitted a cloud of light volcanic sand. This was diffused by the wind over the whole of the circumambient atmosphere, and from the edges of this cloud, where the lightest and finest particles only of the sand were present, frequent coruscations of lightning appeared to emanate, whilst in its denser and blacker portions none such were discernible.

Towards the close of this eruption there occurred a phenomenon which may perhaps be attributable to the volcanic action going on under Vesuvius. In a pond belonging to a private individual at Puzzuoli, all the fish suddenly died. In the lake of Fusaro, at this time, from twelve to thirteen hundred weight of fish were calculated to have perished; and it was remarked, that the victims principally belonged to those species which congregated at the bottom of pools, such as eels. Thus, too, a vast number of oysters at the bottom of this lake were found dead, whereas those which had attached themselves to the stones or the reeds on its sides are said to

have escaped. In the neighbouring lake of Licola, also, several of the same species of fish were found to have perished.

After the 29th of August no further signs of internal commotion were exhibited by the mountain during the past year, except that disengagement of aqueous and aëriform vapours from the crater which is scarcely ever entirely absent.

So tranquil a condition of the volcano, although to a general observer it might appear deficient in that lively interest which belonged to the state of things that had preceded it, was at least favourable to a detailed examination of the several parts of the mountain, and allowed of my descending twice into the interior of the crater, which, owing to the falling in of the two conical hillocks alluded to, presented at that time a comparatively level surface. There were, indeed, three depressions or pits of considerable depth in the midst of it, which, though without any visible communication with the interior, were so charged with the noxious vapours evolved from an infinity of minute and scarcely visible spiracles, that it was judged unsafe to venture down into them. The rest of the crater, however, was a concavity of no great depth, which was traversed by my guide and myself with comparative facility, after we had remained within its precincts time enough to collect the various sublimations that lined its walls, and to condense some of the vapours still copiously exhaled from its crevices. The sides of the crater consisted of strata which might be traced for a considerable way round its brim in a direction nearly horizontal, except in one part, where, from some shock or fracture, they had sunk abruptly downwards. These strata consisted of loose volcanic sand and rapilli, coated with saline incrustations of common salt, coloured red and yellow by peroxide of iron, and presenting a beautiful and brilliant appearance. I could perceive no dykes intersecting these strata, as at the Monte Somma.

In order to collect the vapours, I caused to be constructed an apparatus consisting of the head of a large alembic fitted on to a cylindrical vessel of tinned iron with riveted joints,

which, being open at bottom, and introduced a little way into the ground, served to conduct the exhalations into the receiver connected with it above. By this contrivance I succeeded in the course of an hour or two in condensing a sufficient quantity of the vapour for chemical examination at Naples. In the liquid collected I could detect no saline ingredient, and there appeared only a slight trace of sulphurous or sulphuric acids. The principal body condensed along with the steam was muriatic acid, which was uncombined with any base.

Whether carbonic acid might be disengaged from the crater I could devise no unexceptionable method of determining; yet by comparing the quantity of carbonate of barytes precipitated, by exposing a given quantity of barytic water for five minutes in the vapour of one of the Fumaroles, with what was obtained from the same quantity in equal times exposed to the open air out of the Fumaroles, I am led to conclude that this gas was exhaled.

Of nitrogen, the air of the Fumarole appeared to contain the same proportion as atmospheric air does in general.

No sulphuretted hydrogen was emitted from the crater, neither could I discover, either in the condensed vapour or in the sublimations lining its walls, any trace of muriate of ammonia.

Muriatic salts principally were detected among the latter, but sulphates of lime, alumina, and iron were likewise present.

The next point in the volcano which arrested my attention was the vent on the eastern side of the great cone, from which issued one of the principal streams of lava that burst from the mountain in August last.

The vapours here collected appeared to agree in composition entirely with those from the interior of the crater, and the sublimations were of the same nature, with the addition of much specular iron ore and some muriate of copper.

The lava which had been emitted in August, continued, when I visited it in November, to give out throughout the whole of its course white vapours; and even after the copious

rains which fell subsequently, many of the spiracles, so late as the end of December, continued to emit the same. The interior of the current appeared also at both these periods to retain a considerable proportion of its original temperature. After removing about six feet of loose scoriæ, I at length reached the upper surface of the bed of lava itself, into which it would have been impossible to penetrate without the assistance of mining implements. The surface temperature of the lava was indeed not high enough to melt lead, but one of Daniell's pyrometers, with an iron rod left in contact with it for a few minutes, rose more than one degree. It is probable, however, that I had failed in this instance in obtaining the full temperature of the superficies; for nearly a month afterwards, that is, late in December, after much rain had fallen, I removed the scoriæ from another contiguous portion of the bed, and found that a thermometer placed upon it, and merely covered over with a little sand, rose to 390° of Fahrenheit. From the cracks and cavities of this lava much aqueous vapour was still exhaling, and this I succeeded in condensing by means of the same apparatus which I had employed within the crater.

The condensed steam on examination was found to be impregnated, not only with free muriatic acid, but also with muriate of ammonia; and as the vapours were collected at the very point of their escape from the lava, it can hardly be doubted, that the latter salt was actually present ready formed within the cavities of the stone, having been emitted from the volcano along with the lava itself. The scoriæ which covered the surface of the bed were in some places quite incrusted over with beautiful crystals of this sort, some of which were perfectly white, whilst others were of an orange-yellow colour. The latter appeared to be owing to the presence of oxide of iron. The quantity of sal ammoniac was large enough to repay the trouble of collecting, and much of it was carried away by the peasants to Naples to be sold to the workers in brass and jewellery. Muriate of soda was also common amongst the substances incrusting the scoriæ, but none could be detected in the vapour emitted at the period of my examination.

The very same substances I found to be exhaled, during my stay at Naples, from the crater of the Solfatara of Puzzuoli, which differed, however, in one respect, namely, in that of emitting much sulphuretted hydrogen, from which the vapours of Vesuvius were entirely free. Hence the film of minute crystals of sulphur which forms on the surface of the rock of the Solfatara in the immediate neighbourhood of the Fumaroles; whilst from the Vesuvian lava no sulphur in any form was given ont at the time of my visit, although amongst the sublimations produced at an earlier stage of the operations, crystals of this body were not uncommon.

The disengagement of such principles as water, muriatic acid, and sal ammoniac from a semi-extinct volcano like the Solfatara, is much more intelligible than its escape from the substance of a bed of lava which has already undergone consolidation.

In the latter instance, what is the condition in which we are to imagine such bodies to exist in the heart of the mass? Not certainly in a state of chemical union with its constituents, for we cannot conceive any affinity inherent in salts of ammonia or soda for the earthy ingredients of a bed of lava; neither, if in combination with them, would they be separated, as the latter parted with its heat.

It seems necessary to suppose, that these bodies, being thrown up at the time of the eruption from the interior of the volcano, become entangled within the interstices of the lava at the same time disengaged; that a portion of what was originally ejected still continues in a compressed state within the cavities of the rock, especially in its interior; and that it is only by slow degrees that it finds means of escape through chinks and crevices to the surface.

We know that many trap rocks contain a portion of water and of muriatic acid, and that the latter body has even been detected in the domite of Auvergne, a volcanic production which, comparatively speaking, must be regarded as of extreme antiquity[a]; so that we may more readily conceive in

[a] I might likewise refer to the existence of carburetted hydrogen in a condensed state in cavities of rock-salt at Wielitzka, and that of sal ammoniac in

what manner lavas of recent origin retain large quantities of the same volatile principles, and even of certain saline substances diffused through their pores and fissures.

Perhaps indeed, although chemical attraction in these cases is out of the question, a certain degree of *adhesive affinity* may have been exerted between the substances exhaled and the walls of the cavities that had contained them. Dr. Faraday, in the Sixth Series of his Researches on Electricity, published in our Transactions, has introduced some pertinent remarks on this kind of influence, referring to it, amongst other phenomena, the operation of platina in determining the union of oxygen and hydrogen in Döbereiner's experiment. Nor, indeed, does it seem improbable, that, as heat exercises a repulsive power not only between the particles of bodies, but likewise between masses of them [b], so likewise a species of affinity may exist between masses of matter even where their particles are not mutually attractive; and that the latter may retard the operation of heat upon bodies possessing intrinsically a considerable degree of volatility, and prevent their entire disengagement all at once from the cavities of the substance which had entangled them.

Be that as it may, it seems certain from the above observations that ammonia is one of the original products of volcanic action in the case of Vesuvius; and it would be easy to extend the same inference to other volcanos,—a fact, I am aware, by no means new, but still one, the circumstances of which seem to deserve investigation, especially, as from the readiness with which nascent hydrogen enters into combination with azote, it might be imagined, that the ammonia was somehow or other generated in the open air, owing to a disengagement of hydrogen from the lava [c].

that of the Tyrol, as facts of the same description. The latter might lead to some speculations with regard to the origin of sea-salt, to which I may perhaps on some future occasion recur.

[b] See Professor Powell's Paper in the Philosophical Transactions for 1834, Part II.

[c] See this question more fully discussed in a Memoir succeeding the present, entitled, "On the Evolution of Ammonia from Volcanos."

I trust, that the having traced it to the vapour directly issuing from the mass effectually dispels such a suspicion, and will serve as an additional argument in support of an opinion I have long entertained, that atmospheric air and water both find their way to the seat of volcanic operations and are alike deprived of their oxygen by certain principles there existing; whilst the residuary nitrogen and hydrogen are evolved, in some cases separately, in others united, in the form of ammonia.

Remarks on the Eruption of Vesuvius in December, 1861. Read at a Meeting of the British Association for the Advancement of Science, Oct. 3, 1862.

THE eruption to which I wish to direct the attention of this Section has already been described by several eye-witnesses, two of whom, namely, Professor Palmieri and M. Pierre de Tchihatscheff, have communicated to the Geological Society brief reports of the most striking physical phenomena attending it, such as the outburst of springs of acidulous and hot water, and the upheaval of the ground at Torre del Greco to the height of 1.12 metre above the level of the Mediterranean.

M. Claire Deville, also, a French savant who has made the gases evolved from volcanos his particular study, was summoned from Paris immediately upon the commencement of the eruption, and arrived in time, if not to witness the outbreak, at least to collect and examine the emanations which were its immediate consequences.

All, therefore, I shall attempt to do in this brief communication, is to point out the facts of greatest novelty which others had anticipated me in recording, and to consider the bearing which they may have on the general theory of Volcanos.

Vesuvius, within the last few years, has entered apparently upon a new phase of volcanic operations. At former periods its eruptions occurred at distant intervals apart, but were distinguished by their violence and magnitude.

Thus only nine eruptions are recorded as having taken place between the commencement of the Christian era and the beginning of the seventeenth century; in the course of

the latter, viz. from 1631 to 1694, there occurred four; in the eighteenth century twenty-two; and in the first half of the nineteenth, viz. from 1802 to 1850, no less than seventeen.

Thus, even allowing for the greater imperfection of records during the Middle Ages, which might have prevented a few of the earlier eruptions from having been handed down to us, there seems to be sufficient evidence of a gradually increasing frequency in the volcanic outbreaks, as we approach the present time.

As to the greater violence of the earlier eruptions, there seems sufficient proof of it in the accounts given us by ancient writers, of the fearful outbreak of A.D. 79, by which Pompeii and Herculaneum were overwhelmed; of that of 204, described by Dion Cassius and Galen, in which the noises produced by the ejection of matters from the crater were loud enough to be heard at Capua; of the third, in 472, which is said by Procopius to have spread alarm even at Constantinople; and of that great one in 1631, which, after a pause of one hundred and thirty-one years, during which the crater had been covered with shrubs and rich verdure, overspread with lava the greater part of the villages lying at its foot on the side of the Bay of Naples, and occasioned the death of four thousand persons. But it is further remarkable, that the greater number of these eruptions took place either from the crater, or at least at a high level. One only, that of 1760, broke out at a considerable distance from the summit, namely, on its southern flank, about one mile above the Convent of Camalduli.

Within the last few years these conditions appear in a great degree reversed. In the year 1858 an aperture was formed along the south-west flank of the mountain, from which, after a succession of detonations and earthquake-shocks had taken place from its neighbourhood, a torrent of lava suddenly gushed out; and this was followed, a few days afterwards, by the issuing forth of several other vents along the line of the fissure, which also vomited forth streams of molten matter.

This flow of lava continued from various points, all placed

nearly upon one transversal line to the axis of the mountain, for more than a year, so that in May, 1859, when I took my leave of Naples, it was still going on.

Thus the lava stream travelled slowly down the sides of the mountain, in the direction of Resina, and was finally arrested about half a mile above that village.

The exact period of its cessation I have not ascertained, but I believe it was not long antecedent to the outbreak of December in last year, which took place above the town of Torre del Greco, and has been described by Palmieri, Guiscardi, and other local geologists. Here, it must be observed, the vents or fissures from which the lava issued occurred at even a lower level than on the former occasion, namely, not more than half a mile at the most from the level of the sea, and at a height of only a few hundred feet above it. If, therefore, I may be allowed to judge from these two latest outbursts of volcanic energy, it would seem as if the sides of the mountain had become so much weakened by the continued emission of ignigenous matter during so many centuries, that its walls were no longer able to sustain, as before, the pressure of a column of lava equal to the height of the mountain itself, but gave way at a considerably lower level.

The first remarkable feature in this eruption was the sudden upheaval of the coast, for a distance of several miles on either side of Torre del Greco, to the height of 3 ft. 7 in. at that locality, gradually diminishing, both to the right and left, until it ceased altogether.

Thus, the Balani, Patellæ, Ostreæ, and other marine shells that live adhering to the rocks just at the margin of the seawater, were found to be raised 3 ft. 7 in. above it, affording a parallel instance to the famous one of the columns belonging to the Temple of Serapis at Pozzuoli, on the opposite shore. We have here, perhaps, the first well-authenticated instance that can be cited of an elevation of land near Naples caused by, or coincident with, a volcanic outbreak; for the well-known case at Pozzuoli seems rather to prove an oscillation in the level of the land, than a permanently elevatory movement, as the ground had first sunk, then had risen, and

is now apparently sinking again below the level at which it stood at the time of the erection of the temple.

Several cases, indeed, of apparently permanent upheaval are pointed out on the neighbouring coast, but these cannot, like the present case, be referred to any particular volcanic outbreak, and it will therefore be the more interesting to observe, whether the present elevation of the land near Torre del Greco is maintained, or whether the latter again shall subside, after a few years, to its former level.

It has struck me, that the reason why the lava-stream which issued from the fissures on the morning of the 8th of last December was so soon arrested in its downward progress, may have been its flowing into the hollow occasioned by the heaving up of the land along the coast, which took place during the great earthquake that ushered in the eruption, and produced so much damage and alarm in the town of Torre del Greco.

This last eruption has also been characterized by the evolution of certain volatile matters, not hitherto observed, I believe, amongst the products of Vesuvius.

Upon approaching the town of Torre del Greco, nearly a month after the eruption had taken place, I perceived a very powerful and offensive smell of naphtha, which pervaded the whole place, especially in the vicinity of the sea. Its occurrence reminded me of the asphalt met with in the volcanic tuff at Pont du Chateau, near Clermont, in the midst of the genuine volcanic rocks of Auvergne, and probably as a product of similar operations in the Dead Sea; but the most abundant examples of the same phenomenon are to be found amongst pseudo-volcanic rocks, as at Trinidad, and in Sicily, at Macaluba, and at Leonforte.

Another product, now for the first time detected amongst the emanations of Vesuvius, and perhaps having a similar origin, was light carburetted hydrogen or marsh gas, which M. Deville found bearing in the proportion of from 3 to 4 per cent. to the carbonic acid evolved from the fumeroles near the town. In order in some degree to appreciate what this proportion of the gas would amount to, we must recollect that the quantity of carbonic acid disengaged from the

earth during, and subsequently to, such an eruption as the one I am describing, is something so enormous, that the mind can hardly grasp its proportions.

On the day I visited Torre del Greco, which was on the 10th of January, and therefore thirty-three days after the eruption had taken place, the atmosphere throughout the town of Torre del Greco, and over a considerable area on either side of it, was so impregnated with carbonic acid gas that my respiration was sensibly impeded, especially as I approached the level of the Mediterranean. There, indeed, even in the open air, the oppression on the lungs caused by the presence of this gas was so great, that I was glad to make a hasty retreat to a higher part of the town in order to breathe a purer air. The gas was bubbling up in various places in the sea like a great cauldron, and a copious spring, fully charged with carbonic acid, had appeared in a new place, and was gushing down into the sea close to the hot mineral waters of Torre. No wonder, therefore, that in confined situations, as in cellars, the accumulation of noxious gas was at this time such as to render the atmosphere utterly unrespirable, and that many of the dwellings of the town had been in consequence deserted.

Curious to obtain some rough estimate of the proportion of carbonic acid which pervaded the air of the town and its vicinity, I prevailed upon M. Deville to analyse the latter in different positions, and obtained from him the following report respecting it:—

1st. In the first street, on entering the town from the side of Naples, and at a height of about 30 ft. above the sea's level, at a little distance from a fissure from which mephitic gas was issuing, having a temperature of 40 Cent., the carbonic acid bore as high a proportion as 6·5 per cent. to the remaining air. There the houses were uninhabited, but men were working in the open air, within a few yards of the spot from which the air had been taken.

2nd. In a street which runs at right angles to the former, and at a height of about 5 ft. above the ground, where there was a free circulation of air, the percentage of carbonic acid amounted to 3·1.

3rd. On the road to Resina, outside of the town of Torre, on the slope of the hill upon which it is built, 5 ft. from the ground, under a shed standing in front of a cook-shop, the percentage was 2·6 [a].

I think it might be possible, by applying the formulæ contained in Bunsen's Gasometry to the data thus afforded, to approximate to the quantity of carbonic acid emitted from the ground in a given time, assuming the atmosphere to be impregnated to this amount to the height of 20 ft. from the ground, over an area of a mile, embracing Torre del Greco as its centre, and this state of things to continue for at least thirty-three days from the date of the eruption; but without entering into such calculations, the amount emitted will be seen to be something prodigious, if we estimate the rapidity with which a gas spreads itself through the atmosphere, when no natural obstructions occur to prevent its diffusion. In setting down, therefore, the proportion of marsh gas to that of carbonic acid at 3 or 4 per cent., we in reality represent it as constituting no insignificant product of the volcanic operations going on in this locality.

But how are we to account for the presence of this new gas, and of the naphtha which accompanied it, amongst the emanations of the volcano? Are we to suppose the volcanic processes themselves to have undergone a change, or are we to account for it by their having been set up in connection with certain new materials?

The former of these explanations would probably be preferred if we adopted the views of M. Deville, and recognised with him two classes of volcanos,—the one those of the common kind, the other such phenomena as are exhibited at the Lago Naftia and at Macaluba in Sicily, as well as in the peninsula of Taman, and in some other localities, and which are the results of what persons imbued with this hypothesis have designated by the name of mud-volcanos.

These latter are broadly distinguished from the former by the absence both of lava and of scoriform masses, as well as by the ejection of semiliquid mud, consisting of a kind of

[a] In the most densely crowded apartments, the percentage of carbonic acid has seldom been found to range higher than about 1 per cent.

unctuous clay mixed up with water, having crystals of pyrites disseminated, and a saline efflorescence on its surface. And whilst the erupted masses of an ordinary volcano reveal a temperature sufficient, in some instances, to fuse cast-iron and copper, the outburst of a mud volcano is attended with comparatively little heat; for the ejected mud of Taman is stated by Pallas to have issued quite cold, and the gases of Macaluba were found by Deville to exceed only by 3° Cent. the temperature of the surrounding air.

Moreover, whilst the gases evolved from volcanos in general, during their active condition, are muriatic and sulphurous acid, those which accompany the outbursts of what are called mud volcanos seem to be confined to carbonic acid, light carburetted hydrogen, and nitrogen. This composition, which I determined at Macaluba so long ago as the year 1825 [b], has been also assigned to them by M. Deville in one of his letters to M. Dumas, as the result of his recent examination of this locality.

There seems, therefore, nothing in common between the gaseous products of ordinary volcanos and those of which Macaluba and Taman are the types, except it be carbonic acid, which is emitted in enormous quantities both by the one and the other.

If, then, we were to adopt the hypothesis above suggested, it must be imagined that Vesuvius is at present in a kind of transition state, passing, as it were, from its ordinary phase of operations into one which approximates more nearly to those of mud volcanos; carburetted hydrogen, naphtha, carbonic acid, and azote, taking the place of hydrochloric and of sulphurous acids.

But another mode of explanation suggests itself to my mind, which seems less encumbered with difficulties, and which, whilst it places the pseudo-volcanic phenomena of Macaluba and the like under an entirely different category from those of genuine volcanos, will enable us to account for the occasional occurrence of such products as have exhibited themselves for the first time at Vesuvius, with-

[b] See my "Sketch of the Geology of Sicily," in the Edinburgh Philosophical Journal for 1826.

out supposing any essential change in the character of the operations of that volcano. On looking at the table suspended in the room[c], which states on the authority of Deville, Bunsen, and Boussingault, the nature of the gases disengaged from those volcanos which have been most accurately explored, it will be observed, that some, such as hydrochloric and sulphurous acids, together with, in certain instances, an inflammable gas—which, as it gives rise to flames, probably contains, as one at least of its constituents, hydrogen—occur during a period of intense activity; whilst others, such as carbonic and sulphuretted hydrogen, and sometimes atmospheric air, with less than its normal proportion of oxygen, are disengaged where the action is more languid. Now, I would regard the former as the primary and essential concomitants of volcanic action, the latter as the secondary and accidental ones.

The former gases originate from the chemical actions which either originate, or are inseparably connected with, the internal processes or workings of the volcano.

To my mind they suggest, that the access of sea-water to the seat of the internal action is the prime mover of the processes going on, and at the same time indicate the existence of a heat sufficient to disengage from the chlorides contained in the sea-water their electro-negative principle, leaving the bases free to combine with silicic acid or other earths, and thus to form silicates, aluminates, &c.

They also indicate the existence in the interior of the earth, near and about the seat of the volcanic action, of a deoxidizing as well as of an oxidizing process; the former causing the water present to be decomposed into its elements, and its hydrogen eliminated; the latter causing the sulphur to be converted into sulphurous acid gas, and perhaps other elements, existing in the interior of the earth, either in a free state or in combination with sulphur, also to undergo oxidation.

That these two antagonistic processes should be going on at the same place and time cannot indeed be supposed; but if we grant the existence in the interior of the earth of mate-

[c] This Table is given at the end of the Memoir.

rials capable of decomposing water, it is quite conceivable that the heat produced by this reaction should occasion the volatilization of the sulphur present, and its consequent escape into a region where it could combine with oxygen, and thus be converted into sulphurous acid gas.

But as there are doubtless many who may prefer to imitate the caution of M. Deville, and to abstain from theorising on the subject, I would only ask my hearers to admit with me the essential connection of the above gaseous products with volcanic action, as evinced by their frequent, if not their constant co-existence, apart from any hypothesis as to the cause of their being so associated.

It is different, however, with some of the other gases which will be seen enumerated in the table alluded to[d].

Carbonic acid, though, as we have seen, disengaged in enormous quantities from the earth in the vicinity of the volcanic outbreak, is not in general emitted from the crater itself during the period of an eruption, nor is sulphuretted hydrogen usually detected, except at the foot of the mountain, or during the more languid phases of its action.

The same remark would seem to apply likewise to the petroleum or naphtha, which was so abundantly disengaged after the late eruption, as well as to the carburetted hydrogen now for the first time detected. With the exception, therefore, of the sulphuretted hydrogen, which will be afterwards considered, I am tempted to regard the latter products as due merely to the action of the volcanic heat upon certain materials, upon which it was brought to operate in the neighbourhood of the volcano.

Let us, for example, suppose the Apennine limestone, which we know to occur in immense masses in the immediate neighbourhood of Vesuvius, to contain imbedded in it beds of bituminous shale, or even to be impregnated, as our own carboniferous limestones frequently are, with the same ingredients, and we can then readily understand, that there should be a disengagement, not only of enormous volumes of carbonic acid, due to the heating of the limestone itself, but also of naphtha and carburetted hydrogen,

[d] Page 13.

arising from the slow distillation of the bituminous matters imbedded or contained within it.

As for the sulphuretted hydrogen indeed, so abundantly given off in the precincts of most volcanos, it appears to have a different origin. Its absence from the immediate focus of volcanic heat may be accounted for, as both its constituents would at such a temperature take fire so soon as they came into contact with oxygen; but the sulphur disengaged from the volcano, whether alone or in combination with hydrogen, would form sulphurets with the earthy materials which it met with. Wherever the absence of oxygen admitted of this reaction taking place, it is quite easy to understand that the mere approach of water to these sulphurets should give rise to the evolution of sulphuretted hydrogen.

It is even possible that the sulphuretted hydrogen, found in the immediate neighbourhood of volcanos, may in some instances be derived directly from the volcano, having escaped to the surface through channels in which oxygen was not present in sufficient abundance to cause its combustion to take place. By adopting this hypothesis, we get rid of the necessity both of imagining Vesuvius to be passing into the condition of a Macaluba, and also of admitting any connection or analogy between this volcano and those others to which the name of mud-volcanos has been applied. The latter probably originate in the accumulation of vast beds of earthy and metallic sulphurets, together with bituminous materials, in the interior of the earth, often brought together, no doubt, through the instrumentality of antecedent volcanic operations; but the immediate cause of their eruptions must be sought in the access of water to such materials [c], by which a heat would be produced sufficient to cause the extrication of carburetted hydrogen, as well as of carbonic acid, from the incandescent mass. Hence would arise a heaving up of the semi-fluid mud, the ejection of stones, and even at times the emission of flames. Large as the scale may be on which these operations are going on in the neighbourhood of the Sea of Azof, there is nothing in the nature

[c] See Bischof, Chemical Geology, p. 325.

of the phenomena themselves there exhibited which should justify us in identifying them with ordinary volcanic processes.

The volcanos of Central Tartary, of which we have heard so much, but know so little, may probably turn out to be due to operations of the same nature as those of Macaluba, or of the peninsula of Taman. The most recent and authentic accounts transmitted to us certainly tend to dispel the notion that any true volcanos exist in that quarter[f], so as to establish a real exception to the general rule, that all such operations are dependent upon the near proximity of the sea.

On the other hand, it is difficult altogether to reject the testimony of so many Oriental writers, who speak of burning mountains, and of sal ammoniac and other volcanic products, as common in these regions.

Is it not more probable—as being more consistent with analogy—that phenomena like those which, on a small scale, were presented in the neighbourhood of Lulworth some years ago, and which, in this enlightened age and country, were dignified by the name of volcanic,—phenomena which, on a scale of greater magnitude, have even produced certain not unimportant physical changes upon the condition of the neighbouring country (as in the peninsula of Taman),—may also have taken place in parts of Central Tartary, and have given rise to the accounts that have come down to us. Such an explanation does not preclude the idea that real volcanos may have existed there at some former period, when perhaps a great Mediterranean Sea connected the Caspian with the Lakes of Aral and Baikal; and hence may have arisen the volcanic appearances, of which Erman speaks, in the neighbourhood of the latter; whilst, if this be the case, we should have an adequate cause assigned for that accumulation of sulphurets which, in conjunction with bituminous or carbonaceous matter, would be competent, at any subsequent time, to give rise to the phenomena of the so-called mud-volcanos.

[f] See Meyer's Translation of a journey made by Schrenk, a Russian traveller, in 1840, into the Eastern Kirghisian Steppes, the statements in which narrative were confirmed by the same explorer in 1841.

I offer these remarks with the diffidence due to their speculative and hypothetical character; but what I consider of much greater importance, and should wish to see undertaken, if possible, in every locality where volcanos exist, is an accurate examination of the gaseous and other emanations proceeding from them in their various phases of activity. Possibly, indeed, I may somewhat over-estimate the value of this investigation from having taken some part in it myself, and by finding the results of my somewhat coarse methods of analysis confirmed by the greatly more precise and extended researches subsequently carried out by Bunsen and Deville. But at any rate, I am quite sure that no theory of volcanos can be considered worth attending to, in which an accurate account is not taken of the gases evolved, and in which their occurrence at the time and place at which they manifest themselves is not fully accounted for.

When this Association, some years ago, wished to become better acquainted with the processes going on in the interior of our iron furnaces, in spots unapproachable from their excessive heat, it commissioned Professors Playfair and Bunsen to examine the gases that escape from the upper orifices of their chimneys. And in like manner I conceive we can in no other way become acquainted with what is going on at the focus of a volcano, in spots inaccessible, from their depth, to man, than by collecting the products of the chemical processes there enacted from the fumaroles by which they communicate with the surface.

Perhaps, indeed, I may take the liberty of suggesting to geologists, that in their eager haste to class volcanic movements amongst the consequences of some of those great cosmical changes which are supposed to be going on, they have been sometimes too apt to ignore the chemical phenomena which accompany these great outbreaks, and are not sufficiently alive to the fact, that where chemical operations constitute so large a part of the problem, the aid of the chemist must be invoked, in order to arrive at an adequate and satisfactory solution. Should this be the case, the above remarks, even if they should fail of their direct object, will not be thrown away, since they may tend to direct the at-

tention of those geologists who make volcanos their study, to the real nature of the investigation, and to the methods of research which must be resorted to with a view to its successful prosecution.

TABULAR VIEW OF VOLCANIC EMANATIONS.

No. I.

Volcanic Emanations, classified according to their position with reference to the Volcano in which they occur.

Locality.	Composition. Water.	O	Other Constituents.
Vesuvius (D.)			
Its Crater	absent	norm.	HCL SO_2
— Base	present	def.	CO_2 SH a trace
— Lavas	absent	norm.	CO_2 SH with or without NH_3
Ditto	present	def.	HCL SO_2
Phlegrean Fields			
Solfatara	present	def.	CO_2 SH; CO_2; or SO_2
Lago d'Agnano	present	norm., or slightly def.	CO_2
Lipari Group (D.)			
Island of Volcano			
Crater	present	def.	Flames, SO_2; BO_3; SO_2
North Flank		def.	SO_2
Base	present	abs.	Co_2
Boiling Springs		abs.	SH
Etna (D.)			
Crater		norm.	HCL and SO_2
Base, from Springs		abs.	SO_2
Iceland (B.)			
Hecla, Crater	absent	def.	
Krisiwik, Solfatara		abs.	CO_2 SH; sometimes H
Equinoctial America (Bouss.)	present		
Fumaroles, various	present	norm.	CO_2 SH

No. II.

Volcanic Emanations, classified according to the successive periods of their appearance.

First stage of activity,

From the fissure of the eruption.

No water, atmospheric air, with or without salts containing Cl.

Second stage of activity,

From the lava stream, when just cooled upon the surface, but chiefly from its lower portions.

Water, sal ammoniac, and other chlorides, with atmospheric air.

Third stage of activity,

From the crater above the point whence the lava had issued.

Chiefly atmospheric air, O rather deficient; sometimes with water, HCL, SO_2.

Fourth stage of activity,

From another spot in the crater, above the point aforesaid.

Water, with a bare trace of SH and of S.

Fifth stage of activity,

Found about Etna, but not at Vesuvius.

Water alone.

Sixth stage,

Only appearing towards the close of an eruption, but continuing afterwards during all the subsequent stages of languid volcanic action, the gases being evolved, not from the lava, but from the interior of the earth.

Water, O deficient or wanting, sometimes CO_2, with or without SH.
sometimes SO_2, with or without BO_3.

N.B.—To this latter class belong thermal waters, mofettes, and other obscure results of volcanic action. It is a significant fact, with reference to the theory of volcanos, that whenever water is disengaged from them, the atmospheric air that accompanies it is either wholly, or in part, deprived of its normal proportion of oxygen. This is the result of the examination made both by Deville and Bunsen, neither of whom certainly were biassed by any theory to which such a fact might lend support.

ABBREVIATIONS.—N, Nitrogen; O, Oxygen; O, Norm., Proportion of oxygen the same as in common air; O, def., Proportion of oxygen less than in common air. HCL, Muriatic acid; SO_2, Sulphurous; CO_2, Carbonic; BO_3, Boracic; SH, Sulphuretted hydrogen; NH_3, Ammonia; NaO, Soda; KO, Potash; (D.), St. Claire Deville; (B.), Bunsen; (Bouss.), Boussingault.

On the Elevation Theory of Volcanos,

In Reply to a Paper of Mr. Poulett Scrope, read before the Geological Society, Feb. 2, 1859;

Being the Substance of a Communication made to Section C., at the Meeting of the British Association for the Advancement of Science, held at Oxford in 1860.

WHEN Sir Charles Lyell, in the able Memoir he published in the "Philosophical Transactions" for 1858, had exposed, by a train of carefully conducted observations, the fallacy of M. Elie de Beaumont's position, that sheets of compact lava could have been formed only upon gentle slopes, I, for one, was thankful to him for being enabled to extend to all that portion of Vesuvius which falls under our review the same mode of formation which we see illustrated in the more recent of its beds produced within our own memory. Whatever may be the case with regard to the nucleus of the mountain which lies concealed from our sight by innumerable sheets of superimposed lava, one was naturally glad to fall back upon the simpler notion as to the building up of a volcanic mountain by the successive outbursts of beds of lava and showers of scoria, which the older geologists had espoused, so soon as it had been shewn that the high inclination which many of these beds assume constituted in fact no valid objection to this mode of explaining their origin.

And although nothing can be more illogical than to regard the removal of an objection to an hypothesis as in itself tantamount to a proof of its validity, I cannot wonder that the establishment of the fact, that lavas have been consolidated at high angles, should contribute to give a preponderance in the eyes of geologists to the eruption theory,

and to bring into comparative discredit the opposite one of elevation.

So much, indeed, has this been the case, that it seems to be assumed by many in the present day, that the latter hypothesis is as completely put out of court by the late researches of Sir Charles Lyell, as the opposite one was supposed to be about ten years ago by the investigations of M. Elie de Beaumont; at which period, Mons. Dufrenoy, with a full knowledge of the current opinions of the geological world, took upon himself to affirm, that although formerly it had been imagined that a volcano was built up by successive eruptions of lava and ejections of ashes, no one at that day would venture to maintain so extravagant a position.

To prevent these and similar revolutions of opinion from being quoted against geologists as proof of the shifting basis upon which their theories are founded, we might do well to bear in mind, that although it may be perfectly legitimate to extend, so far as we are able, to volcanic rocks in general that mode of formation which we see going on before our eyes in volcanos now in activity, and to abstain from going farther for an explanation, until we have satisfied ourselves that the former is inapplicable to the particular circumstances of the case we are contemplating, yet that, considering the great variety of circumstances under which volcanos have been formed, and the numerous phases of action they display in their operations past and present, it would seem hasty and presumptuous to assume, that the mode of formation which we witness in a few familiar cases should be applicable to all the remainder.

It is on this account that I feel myself called upon to notice the Memoir of Mr. Poulett Scrope, published in the "Quarterly Journal of the Geological Society" for November 1859, which—but for the too dogmatic tone it adopts, and the confidence with which it treats as exploded fallacies the opinions of such men as Humboldt, Von Buch, Dufrenoy, and the like,—I should have hailed as presenting an useful summary of the arguments that may be alleged against the elevation theory, as well as a warning against following too

implicitly the guidance of the French school of geologists in their interpretation of the phenomena of volcanos.

But Mr. Scrope does not rest satisfied with this more practical and less ambitious aim, but would wish to persuade us that no such event as the sudden rise of a volcanic, or, I presume, by parity of reasoning, of any other mountain whatever, has or ever can have occurred; thus placing me—as the author of a general work on the subject of Volcanos—under the alternative, either of abandoning as unsound the arguments upon which I therein maintained the general principle of elevation, as applicable to the case of volcanos, or of attempting to shew, that they remain in a great degree unshaken by his reasoning.

Some indeed may apply to my speculations the same remark which Mr. Scrope has himself suggested, to account for the want of attention paid to his own by those great authorities who still, in defiance of them, continued to the end of their days to maintain the elevation theory; and I may not perhaps be wrong in supposing, that in pursuit of higher game he has overlooked the arguments advanced in my more humble volume.

Still, as he has done me the honour of coupling my name with those of M. Elie de Beaumont, Baron Humboldt, Von Buch, and Professor James Forbes, as amongst the number of those who have advocated, at least to some extent, the elevation theory, it might have been expected, that in so elaborate a paper he should have touched upon the principal arguments in favour of that view contained in my work on "Volcanos." On the contrary, it would appear as if Mr. Scrope imagined, that the only direct proof ever offered of the elevation of a volcano was the celebrated case of Jorullo; for, with respect to that of Methone in Argolis, alluded to by Ovid, he dismisses it as a poetic myth, forgetting that the fact of the heaving-up of this promontory is vouched for by many prose writers of antiquity, and especially by the accurate Strabo, whilst it is countenanced by the observations of those who had visited the spot recently, as by Virlet and Boplaye, in their account of the French expedition to the Morea.

But passing this over, let us proceed to consider the circumstances of Jorullo, where we are told that a tract of ground, from three to four square miles in extent, became elevated 524 feet above its former level; whilst, from the midst of the swollen or upheaved mass, six conical hills, varying in height from 300 to 1,600 feet, appeared in the course of a single night, rising above the original level of the plain.

This phenomenon, which Baron Humboldt, after an inspection of the spot, pronounced to be owing to an upheavement of the country by subterraneous agency, Mr. Scrope, without having visited the locality, undertakes to account for by the mere pouring out of lava from a volcano, formed, like the Monte Nuovo, by a sudden outbreak of volcanic energy.

He remarks, that some of the lava streams of Iceland equal in thickness the ground upheaved at Jorullo; alluding, no doubt, to the great eruption of Skaptaa Jökull in 1783, but overlooking the fact, that the stream in this instance, when it attained that depth, was pent up within a narrow gorge, on emerging from which, and arriving at a country where it had liberty to expand, its depth never exceeded 100 feet.

He also maintains, that if the lava was only imperfectly fluid, and if the surface over which it flowed had been quite level, there is no reason why it might not have been circumscribed within the limited area which it actually covers.

But the difficulty in accepting such a solution, which presents itself to my mind, is that of conceiving the possibility of any mass of matter, sufficiently approaching to fluidity to have descended with such rapidity the steep incline of the central conical hill—to which Mr. Scrope traces it—being arrested in its downward course at so short a distance from its point of issue, as to have merely mantled round the base of the volcano in the manner which Humboldt represents.

It strikes me that, if the lava were fluid enough to have flowed down the sides of the mountain, and to have reached its base in the short time recorded, it must have acquired an impetus which would have propelled it onwards, in one direction or another, for a considerable distance, so that the

limited area it occupies at the foot of the mountain is to me the best evidence of its not having descended as a current from the eminence to which Mr. Scrope traces it. Mr. Scrope, indeed, cites as a parallel case, on the authority of Postel and Linz, the great volcano of Awatscha in Kamtschatka. This mountain, it seems, emitted a stream of lava, which, when it reached its base, was so speedily arrested in its progress, as to form a sort of promontory of considerable height, jutting out from the flank of the volcano.

But the parallel fails in two respects—*first*, because we do not know what period of time had been occupied in the piling up of this mass of lava; and *secondly*, because the latter is confined to a single spot, and does not mantle round the base of the mountain, as is the case with Jorullo. Without, indeed, adopting those peculiar views with regard to the fluidity of lava which Mr. Scrope ventured upon in his earlier publications, I can readily admit that, like glass or pig-iron, the products of volcanic operations may exist in every degree of viscosity, from a state not far removed from the solid, to one admitting of a free motion of the particles; and hence I can the more readily understand the upheaval of large masses of ignigenous materials in a pasty or semi-fluid condition. But I cannot so well imagine a lava-current, fluid enough to descend rapidly the slope of a mountain, being arrested suddenly at its base, or covering the level ground immediately encompassing the latter, without invading the territory beyond.

In the present state of our information, therefore, most geologists, I believe, would be disposed to accept the original hypothesis of Humboldt as less open to objection than the one which Mr. Scrope has proposed in its place; and although it is possible that subsequent investigations may lead to a different interpretation of the phenomena, yet they must proceed from persons who have visited the spot, and not from speculators at a distance.

I shall be glad, therefore, to learn the results of the journey which it appears M. de Saussure has recently made to the locality. He is said by Mr. Scrope to have convinced himself of the erroneousness of Humboldt's theory; but

before he can expect us to adopt his conclusions, he must be prepared to shew, either that the facts which the Prussian philosopher has recorded with respect to this volcano are themselves untrue, or that they are reconcilable with the march of an ordinary eruption.

Humboldt, it is true, was not an eye-witness of the eruption he describes; but the sudden elevation of a tract of land is an event to which the inhabitants of the neighbourhood, upon whose authority he records it, would have been as competent to bear testimony as the most scientific observer.

Now, that which gives the peculiar significance to the case of Jorullo is its affording a key to the formation of those numerous volcanos which have from time to time been elevated in the midst of a deep sea, to none of which, I may remark, Mr. Scrope has alluded.

Of islands raised by elevation, I may enumerate the following, as having occurred within historical times :—

1. The rock which Langsdorff describes near the island of Unalaschka, in the Aleutian group, 3,000 feet in height, consisting of trachyte, which made its appearance in 1793.

2. The island of Sabrina, near St. Michael's, in the Azores, about a mile in circumference, and from 200 to 300 feet above the level of the ocean, which rose suddenly in the midst of the sea, and after continuing in sight for some weeks, again disappeared.

3. The island of Santorino and its appendages, in the Grecian Archipelago, which have been thrown up on various successive occasions, the earliest event of the kind recorded in authentic history being 197 B.C., according to Pliny and others. The next event of the kind happened A.D. 96, in the reign of Claudius; the third in 1573, when the rock of Little Cammeni was thrown up; and the last, the one recorded by Father Goree in 1707, when a new island arose between the Great and the Little Cammeni. Professor Edward Forbes, the last scientific traveller who visited the spot, states his own impression to be, that these islands together constitute a crater of elevation, of the walls of which the outer ones are the remains, whilst the central group is of later origin,

and consists partly of upheaved sea-bottoms and partly of emptied matter, poured forth, however, beneath the surface of the water. He further informs us, that the shells which he collected in the bed of pumiceous conglomerate, constituting the mass of the island observed by Father Goree in the act of rising, consisted of species which could not have lived at a less depth than 220 feet below the surface of the water, thus shewing the extent of elevation to which this rock had been subjected [a].

4. The island thrown up near Iceland in 1783, about 30 miles south-west of Cape Reykianes, which sunk again within a year after its elevation.

5. The phenomenon which occurred off the coast of Sicily in 1831, when an island 3,840 feet in circumference, and rising 107 feet at its highest point above the sea, suddenly appeared on the 13th of July, and sunk again in the latter end of December. It was first visited by Captain Swinburne, who gave it the name of Graham's Island; and was afterwards explored by Dr. John Davy, M. Constant Prevost, and others.

I have enumerated all the cases of sudden elevation that have occurred within the memory of man in the midst of deep water, because, although passed over by Mr. Scrope, they do not seem to me easily explicable by the common hypothesis. If produced merely by an accumulation of loose masses thrown up from some submarine vent, it might be expected that their appearance would have been less sudden, and their slope more gradual.

In the instance of Graham's Island, it would appear that the soundings round the coast rapidly sunk from 1 to 40 or 50 fathoms. Had the island been built up by a gradual accumulation of loose fragments, ought not the sea for many miles round to have had its depth diminished in consequence?

Moreover, it is recorded by Dr. Davy, that whilst the general temperature of the sea was at the time 80°, that immedi-

[a] A further upheavement of the bed of the sea near Santorino took place in 1866. See "Memoirs of the Geological Society," vol. xxii.

ately about the island reached no higher than 70° or 72°, a circumstance which Arago explained by supposing a mass of rock possessing a lower temperature to have been thrust up through the midst of the waters.

If we may be permitted to embrace within the question those numerous volcanic islands which appear to have been derived from outbursts of volcanic energy taking place in the ocean at periods anterior to man's observation, still greater difficulties present themselves in many cases to the application of the received theory. In the island of the Great Canary, for instance, Von Buch describes the nucleus of the crater as consisting of trachyte, which therefore must have risen from the bottom of a deep sea to the height of some thousand feet above its surface. The same is represented to be the case in the contiguous island of Palma, where from the summit of the crater, or the Great Caldera, we look down upon a succession of beds of basalt and of volcanic conglomerate reposing upon a single bed of trachyte, which latter would therefore seem to have been upheaved from the bottom of the ocean to the height at which we now observe it.

If, therefore, it be considered logical to extend to the older lava-beds of Vesuvius the same explanation which we adopt with reference to those found within the compass of our own observation, it would seem not less so to infer that the superincumbent beds of volcanic materials which, in the instances just quoted, rest upon the trachyte, have been tilted up by the same movement which upheaved the latter.

At Teneriffe, Sir Charles Lyell, although disputing Von Buch's elevation theory as applied to that volcano, admits that the whole island may have been raised bodily out of the sea by an upward movement. According to his views, indeed, this movement was a gradual one, a position which no one can gainsay in the case of a volcano elevated before the memory of man, but which seems untenable in the instances before cited, where an island like Sabrina made its appearance in a single night, or, like Graham's Island, in the course of a few days.

Mr. Scrope has also omitted altogether to explain the

formation of those crater-shaped cavities which occur in certain volcanic districts, as, for instance, in the Eyfel country, the elevated borders of which are composed exclusively of the rocks of the country, without any admixture of volcanic matter. Such is the fact with the circular volcanic lake called the Meerfeld; and the same remark, according to Mr. Scrope's own description, applies to that call d La Gour de Tazana[b], in Auvergne.

Indeed, many circular valleys in various parts of the world have been attributed by geologists of high authority to an upheaving of the surrounding rocks, such, for instance, as that in which the acidulated springs of Pyrmont are situated, and those to which Dr. Buckland assigned the name of valleys of elevation in this country. Such events, of course, having taken place at periods of time long antecedent to historical records, cannot be adduced as independent proofs of sudden elevation; but they at least shew that, in interpreting volcanic phenomena, the analogy of nature does not limit us to that one mode of accounting for the formation of such mountains which is exemplified in Vesuvius or Etna at the present day.

Having now alluded to certain facts which have been altogether passed over by Mr. Scrope, I will next notice others which appear to me difficultly reconcilable with his hypothesis.

That a body of semi-fluid materials should have been heaved up by the force of elastic vapours acting from below, so as to form a conical or dome-shaped mass elevated many thousand feet above the level of the contiguous country, and yet, owing to its viscosity, should have been confined to the area which it first occupied, is a supposition not unencumbered, indeed, with difficulties, but at least in no glaring opposition to mechanical or chemical laws; and hence, in such cases as the five dome-shaped mountains in Auvergne, of which, and of the contiguous hills, Mr. Scrope in his beautiful panoramic views has furnished us with so graphic

[b] "Volcanos," p. 48.

a delineation, I should not scruple to adopt it as preferable to any other that has yet been proposed.

But that a body of lava, fluid enough to have been ejected from the interior of the earth as a lava current, should have gradually accumulated round a sort of central nucleus in such a manner as to build up by degrees a mountain of so great an elevation, and yet that it should never have diverged in any one direction, or produced a stream flowing either to the right or to the left, is to my mind as inconceivable as it is unprecedented.

The nearest approach, indeed, to a parallel case which Mr. Scrope has been able to adduce is the pillar of lava forty feet high on the flank of Mouna Roa, which Mr. Dana describes as produced by successive jets of viscid matter congealing one over the other. But the difference between 40 and nearly 5,000 feet is so great, that we must demur as to extending to the latter the hypothesis which we apply to the former, especially considering that we should have to stretch it still farther in order to meet such cases as that of Chimborazo, the whole of which mountain, though no less than 21,100 feet in height, is stated by Humboldt and others to be composed of a species of trachyte, without any vestige of a crater or of ejected materials being found in connection with it.

It was with these gigantic phenomena fresh in his recollection that Humboldt, the great and principal explorer of these extensive regions, conceived himself privileged to protest against theories founded only upon the observation of the volcanos of Italy, and with a pardonable feeling of exultation at the wider field of induction which his own superior opportunities of foreign travel had afforded him, compared the geologist, who imagined all the eruptive rocks throughout the world to be moulded according to the model of those he was familiar with in Europe, to the shepherd in Virgil, who supposed, in the simplicity of his heart, his own little hamlet to contain within itself the image of imperial Rome.

At any rate, in the face of such facts as I have adduced, it would seem the most prudent course, in the present state

of our knowledge, to keep in view the elevation theory as a reserve upon which to fall back, in case any of the phenomena of volcanos should appear upon examination irreducible to any simpler and more familiar hypothesis [c]. The theory of upheaval, indeed, must not be considered merely on its own merits, but as constituting a part of a larger question, which cannot yet be regarded as disposed of.

If the strata of the globe generally have, from time to time, been affected by paroxysmal action, it can hardly be denied that volcanic rocks would be of all others the most likely to come in for their share in these great movements; and although the tendency of the elaborate researches and acute reasonings of Sir Charles Lyell and his school has been to shew, that the slow and gradual operation of subterranean forces may have brought about a vast number of those changes which affect the earth's surface, I am not aware, that the great body of geologists are as yet prepared to admit, that the same forces cannot have operated here and there in a more sudden and violent manner.

Indeed, the more enlarged views which men of science of the present day entertain with respect to the past duration of the globe—to the prevalence of which none have contributed more than Sir Charles Lyell and his immediate followers—prepare us to admit the probability of greater convulsions having taken place than any that we are actually cognisant of.

For what, after all, is the historical epoch within which our experience is necessarily circumscribed, but a mere speck in the series of past events, and therefore one by no means likely to represent to us all the possible phases through which the crust of the globe may have had to pass in arriving at its present condition?

[c] By reference to my work on Volcanos, pp. 622 *et seq.*, it will be seen that this was the point of view in which I contemplated the elevation theory long ago. Although there might be no direct proof that the older lava-beds, which form the bulk of volcanos now in activity, had undergone upheavement, I should even then have preferred supposing them built up in the same manner as the more modern ones, had not, in some cases, their rapid slope appeared at that time to present a difficulty in the way of this explanation.

The laws of nature, indeed, are uniform and constant; but does not our experience of the sudden effects of earthquakes prepare us to expect paroxysmal effects as a part of nature's economy; and do we not perceive as great a difference in its mode of action, when we compare the convulsive energy which, after the occurrence of a succession of earthquakes, elevated a line of coast on the shores of Chili in 1822-23, with the slow and tranquil upward movement which tends imperceptibly to raise the coast of Sweden above its former level, as between the gradual building up of a volcanic mountain by successive additions of lava currents, and scoriæ, and the elevation of a whole tract by a single burst of expansive energy?

The general question, however, as to the probability of paroxysmal action having occurred, is one upon which the opinion of a mere geological amateur like myself will weigh but little; nor, indeed, even in that more limited field on which alone I may be considered as at home—namely, that of volcanos—should I pretend to do more than to caution geologists against the mistake of running from one extreme to the other, and after having adopted to the full the particular views of M. de Beaumont with regard to the formation of craters, hurrying at once to the conclusion, that all the phenomena we observe may be explained by the mere pouring forth of streams of lava and ejections of scoriæ.

Granting that the volcanos we now see in activity may all be referred to this mode of operation, where are the traces to be found of a similar series of phenomena in the older volcanic regions of Mont Dor and Cantal?

For although Mr. Scrope has boldly assigned them all to three distinct volcanos which he assumes to have existed,—namely, those of Mont Dor, Cantal, and Mont Mezen,—yet, as he has not undertaken the task of referring in detail the volcanic rocks of each district to their respective origin, as lava-beds proceeding from one or other of these supposed vents, I must for the present retain my scepticism as to their having been formed in the manner he imagines, and adhere in this instance to the hypothesis suggested by M. de Beaumont, that they were spread almost horizontally over the

surface of the subjacent granite, and afterwards heaved up into the position in which we now find them by some force acting from beneath.

And, if we extend our view beyond the range of those igneous rocks which shew a certain resemblance to the ones produced under existing circumstances, can we feel confident that, amongst rocks subjected to the incumbent weight of many miles of ocean, the same series of operations which we witness in our subaërial volcanos would repeat themselves in an unmodified form?

Is it not rather more likely that, under so enormous a pressure, sheets of ignigenous materials should spread themselves over the bottom of the sea, until, by their accumulation, the repressive action became so great, that it could only be overcome by the elevation of a volcanic mountain to the surface, and by the establishment of a permanent vent?

I offer these considerations as furnishing at least an apology for not withdrawing those passages in my Work which have reference to the elevation theory.

In a descriptive Treatise like my own, the true business of the author ought, I conceive, to be that of placing before his readers, not only the facts which have been established, but also all the tenable hypotheses which have been put forward to account for them; and although even the great names of a Humboldt or a Von Buch would furnish no excuse for clinging to an exploded error, yet the knowledge that such men as these adhered to the end of their days to a certain view with respect to the nature of volcanic operations, should render us more rigorous in exacting from those who reject it the requirement, that all the phenomena could be distinctly referred to some other cause.

I have indeed to express my regret that, owing to absence from England at the time when Mr. Scrope's paper was read before the Geological Society, I had no opportunity of making any remarks upon its substance in his presence; nor could I have done so subsequently before the same

audience, because, as the admission of a purely controversial paper at a meeting of a London scientific society is itself rather unusual, I could not presume to trespass upon the time of the members by bringing forward on a subsequent occasion the arguments that might be alleged on the opposite side, especially as they had for the most part been already laid before the public in my work on Volcanos, of which the last edition appeared in the year 1848.

As, however, some at least of these arguments do not appear to have been noticed in the Memoir referred to, I trust no apology is needed for submitting them to the Geological section of the British Association, where they will receive, I am sure, a fair and impartial consideration.

On the Evolution of Ammonia from Volcanos.

(*From the Proceedings of the Geological Society, Jan. 20, 1858.*)

IN the year 1849, Wöhler ascertained that the copper-coloured crystals found so frequently in the ferruginous mass, technically called "bear" or "horse," which accumulates in the hearths of the iron-smelting furnaces, and which Dr. Wollaston a long time before had pronounced to consist of titanium, were in reality composed of the cyanide and nitride of that metal, containing 18 per cent. of nitrogen and 4 per cent. of carbon.

It has been since found, that the same nitride is also obtained in the process recommended by Rose for procuring the metal, when chloride of titanium together with sal-ammoniac is heated in a glass flask; in which case metallic scales, which had been formerly regarded as consisting of metallic titanium, but which now turn out to be a compound of it with nitrogen, are left in the vessel after the other matters had sublimed.

The facts just mentioned, however interesting to a chemist, would not be of a nature to bring before this Society, were it not for their bearing upon one part of the "Theory of Volcanos," namely on the evolution of ammonia, and the consequent presence of ammoniacal salts amongst the products of their operations.

This, then, is my excuse for proposing to occupy a few minutes of your time on this occasion with some comments upon these facts, and upon the inferences to which they appear to conduct us.

I should, perhaps, in the first instance remind you, that sal-ammoniac, or ammonia combined with muriatic acid[a], ranks amongst the commonest products of volcanic action, and is found in such quantities efflorescing on the surface of newly-ejected lavas at Vesuvius, as to be regarded worth collecting for the purposes of the arts.

[a] Chloride of Ammonium, according to the new views of chemical composition.

The mode of its production has given rise to much speculation; but I shall confine myself to three hypotheses that have been suggested to account for it: namely, to one by Professor Bischoff, of Bonn; a second by Professor Bunsen, of Heidelberg; and a third by myself.

The first of these may, it is presumed, be dismissed in a few words.

It is founded upon the assumption that bituminous, coaly, or other organic matters exist in the neighbourhood of volcanos, by the decomposition of which ammonia comes to be generated [b].

In reply to this, it may be sufficient to remark, that we have no evidence of the existence of such materials in volcanos generally, and that, if the ammonia evolved had been derived from such sources, it ought to be accompanied with bituminous exhalations, with carburetted hydrogen, and with other products arising from the distillation of such materials, which have never been observed to be present [c].

I will proceed, then, to consider the second hypothesis—that of Professor Bunsen; namely, that the lava, in flowing over the herbage existing on the surface of the land which it invaded, had caused the conversion of the nitrogenized matter present in it into ammonia, which, meeting with muriatic acid, a gas constantly present in volcanos when in a state of activity, was sublimed through the crevices of the lava-current in the form of sal-ammoniac.

[b] See his "Elements of Chemical Geology," Engl. Transl., p. 212.

[c] Carburetted hydrogen was stated by myself, ("Volcanos," p. 267,) from personal observations made in 1824, embodied in my memoir on Sicily, published in "Jameson's Journal," to be abundant in certain lakes situated near the base of Etna, as well as at Macaluba in the centre of the island. This M. Deville, in his Memoir, (*Ann. de Chim.*, Jan. 1858, p. 62,) has confirmed, stating with great apparent accuracy the proportion which this gas bears to others present. But these latter evidently belong to the class of "Salses," and must be distinguished from true volcanos, in which carburetted hydrogen has, I believe, never yet been detected.—See my remarks on Macaluba, "Volcanos," 2nd. edit., p. 539.

It appears, however, from the facts stated in the preceding Memoir, p. 18, that carburetted hydrogen as well as naphtha, is sometimes evolved by actual volcanos. It is not, however, accompanied in this case by ammonia, and must be regarded as an exception to the ordinary series of phenomena.—(June, 1867.)

Now, without pausing to consider how far the largeness of the quantity of sal-ammoniac evolved may be consistent with such an hypothesis, I will merely observe that its validity depends altogether upon the question whether the salt is confined to the lava-currents, or has been met with likewise amongst the products derived from the crater itself.

In my Memoir on the eruption of Vesuvius in 1844, published in the "Philosophical Transactions," I have already stated the latter to be the fact; but it had since been denied with reference to the volcanos of Iceland by Bunsen, and disputed in the case of Vesuvius by Scacchi, one of the most distinguished observers of volcanic phenomena at present in Naples.

It nevertheless appears at length to be substantiated on good authority, by the recent researches of Palmieri, with respect to the products of the eruption or series of eruptions, which has been going on for several months past, and which has not yet terminated.

We are therefore driven to resort to some other solution, which must be independent of the supposed presence of organic matter in any form; for undoubtedly nothing of the kind can exist within the focus at which volcanic operations have been for so long a period carried on.

The hypothesis I myself brought forward some years ago to account for the phenomenon, and which assumed that gaseous hydrogen, although incapable of combining with nitrogen under ordinary pressures, might unite with it under that exercised upon it in the interior of the earth, is not open to the same objections as the two already commented upon; but it is unlikely, perhaps, to meet with much countenance, until the experiment of bringing the gases together in a highly condensed condition shall have been tried in an unexceptionable manner.

Perhaps indeed, if, as Professor Bischoff states, a mixture of the two gases has been submitted to a pressure of 50 atmospheres without combining, the question is not likely soon to be set at rest, since it would not be easy to apply an artificial pressure equal to that to which they would be subjected at the bottom of the ocean; where, at the depth of

a mile and a quarter, the pressure is equal to 2805 lbs. to the square inch, or to at least 187 atmospheres[d].

Might not the experiment be tried by sending down to a great depth in the ocean a vessel charged with a mixture of the two gases, furnished with a piston, which should exert upon the contents a pressure equal to that of the weight of water incumbent?

But the affinity which certain metals possess for nitrogen seems to me to afford a more solid ground upon which to build a theory to account for the fact before us.

Not to speak of the simple combustibles, phosphorus and sulphur, and the alkaline metals, potassium and sodium, which form combinations with nitrogen,—zinc, copper, and iron may be instanced as bodies capable of combining with it, —the nitride of the latter even disengagiug ammouia when heated in contact with water.

As these combinations, however, require for their production the previous formation of ammonia, through the medium of which alone they are known to be brought about, they cannot be appealed to as proving that the metal in question is capable of uniting with gaseous nitrogen, and are only cited as indications that nitrogen possesses a wider range of affinities than had formerly been attributed to it.

To meet the requirements of my hypothesis I must go a step further, and appeal to the late researches, which, it seems, that Wöhler, in conjunction with M. H. Saiute-Claire Deville[e], of Paris, has instituted with regard to the metal titanium, undertaken with a view of elucidating the discovery, which he had announced in 1849, concerning the combination which this body forms with nitrogen. Now it appears from the recent researches of these two chemists, reported upon in the *Comptes Rendus* for October 1857, (and which I now find detailed in a more extended form in the *Annales de Chimie* for the present month, January 1858,) that titanium absorbs nitrogen from the air, even in pre-

[d] The pressure of the atmosphere may be estimated at about 15 lbs. to the square inch; the pressure of the sea at $1\frac{1}{2}$ mile is $187 \times 15 = 2805$.

[e] *Ann. de Chimie*, Jan. 1858, p. 61.

ference to oxygen; titanic acid being reduced at a high temperature whenever air is admitted, and a nitride of titanium being formed in consequence. Such, indeed, it is stated, is the affinity of this metal for nitrogen, that titanium can only be obtained pure by heating titanic acid in an atmosphere of pure hydrogen; for if the operation be conducted in the presence of nitrogen, a nitride is sure to be produced. The union, indeed, takes place with so much energy as to generate light and heat, and thus to constitute a genuine case of combustion, in which nitrogen, and not oxygen, acts as the supporter. After the operation is concluded, a metallic matter, of a copper-red colour, sprinkled with brilliant crystalline laminæ, is produced; and, in proof that this contains nitrogen, it may be sufficient to state, that hydrate of potass extricates from it sufficient ammonia to saturate a large amount of muriatic acid.

It is even not necessary to bring the metal into contact with pure nitrogen; for if a closed charcoal-crucible containing titanic acid be kept at a high temperature for some hours, sufficient nitrogen penetrates the porous texture of the vessel to displace the oxygen, and to form with the metal a nitride, capable, as in the preceding instance, of being decomposed by hydrate of potass into ammonia and titanic acid. It is true, that in order to effect a combination between the metal and the nitrogen, the titanic acid must first be reduced, and consequently the presence of carbon in some form or other would appear to be an essential condition. But if such a re-action be conceived as taking place, not near the surface, but at such depth as that at which the advocates of the Chemical Theory of Volcanos would place the still unoxidized, or but partially oxidized, nucleus of the globe, no reducing agent would then be required to bring about the supposed union[f].

[f] This ammonia would, of course, combine with muriatic acid so soon as ever it came into contact with that body; but the experiments of Gay-Lussac, which most chemists have repeated, shew that, at a high temperature, a chloride in contact with silica or alumina has its chlorine set free, whilst its base forms a compound with the silicic or other acids present.

The existence, therefore, even of common salt at the spot where the nitride

Other experiments are given by Wöhler, all tending to establish the same point; but enough probably has been said to shew that, at an elevated temperature, titanium exerts a strong affinity for nitrogen, and that the compound which it forms evolves ammonia under the agency of the fixed alkalies. Now, that titanium is present in most volcanos, is obvious from the occurrence of titanite in several places in Auvergne, at Kaiserstuhl in Germany, at Teneriffe, in Mexico, and amongst the recent as well as the older products of Vesuvius and its neighbourhood. And, if it be objected that it is found in an oxidized and not in a metallic form, the same remark applies equally to all the other bases which are present along with it.

It may, however, be fairly asked, whether its probable quantity within the mountain be sufficient to account for the large amount of ammonia often disengaged?

This, indeed, is a question to which I should be loth to give an affirmative reply; and, therefore, whilst maintaining that the affinity of titanium for nitrogen does furnish us with a *vera causa* for the presence of ammonia amongst volcanic products, I am more disposed to insist upon the fact, as supplying us with an argument from analogy, that other bodies which are present in volcanos may, through a similar re-action, be instrumental in bringing about the same result.

It seems hardly possible that the affinity for nitrogen should be confined to titanium alone; and indeed I have pointed out, that under different circumstances it can be proved to extend to several of the commoner metals.

Is it not, therefore, more probable, that in the interior of the globe, where high pressure and other circumstances may modify the nature of those re-actions which take place under our eyes, nitrogen combines directly with other bodies besides titanium—with iron, for instance, or possibly even

was formed, would not prevent the alkalies from exerting their proper action upon the nitride, and giving birth to ammonia.

The only difference in the result would be, that, in this case, the formation of sal-ammoniac would be immediate, whilst otherwise it might take place during the passage of the gas upwards to the external air.

with hydrogen; and that it is in this manner that that amount of ammonia which often finds its way through the orifices of a volcano may be generated, seeing that its abundance is often such as would lead us to doubt whether the nitride of a metal comparatively so rare as titanium could alone afford it; at least, if the quantity found on the surface in such localities is to be regarded as an index of the proportion which it bears to the other principles present in the interior of the volcano?

P.S.—Since writing the above, my attention has been called by a friend to a more recent paper by M. Ste-Claire Deville, reported upon in the *Comptes Rendus*, which also I find given *in extenso* in the January number of the *Annales de Chimie* for this year, 1858. From this it appears that boron also, like titanium, has the property of combining directly with the nitrogen of the air, and that the compound which it forms with it possesses alike the property of evolving ammonia under the influence of the alkaline hydrates [g].

This fact is the more significant, from the occurrence of boracic acid in the craters of certain volcanos, as in the Æolian Islands, accompanied in this instance, as I have myself found, with muriate of ammonia; and, where the two are thus associated, we should not be disposed to look for the source of the ammonia further than to the nitride of boron generated within the interior of the volcano.

Boracic acid, however, does not seem to be usually present in the lavas of Vesuvius, for it has been only once observed amongst the products of its eruptions, namely, by Monticelli and Covelli in 1817. I should, therefore, be unwilling to attribute the formation of ammonia in this volcano gene-

[g] The discovery of the compound of nitrogen and boron was made in 1841 by Mr. Balmain, who, however, combined the two only by indirect methods. Mr. Warington also, in 1854, (Report Brit. Assoc. 1854, Sections, p. 76,) pointed out the existence of this very combination in the crater of the Island Volcano, suggesting that the sal-ammoniac so abundant in that locality might be due to the decomposition of the nitride by the action of steam. But the more recent researches alluded to in my text first established that this nitride might be formed by the direct union between boron and nitrogen.

rally to the agency of boron, and am rather disposed to appeal to the fact as another proof that gaseous nitrogen, instead of that chemical indifference which has been commonly attributed to it, possesses in fact a somewhat wide range of affinities, and therefore may, without violence to analogy, be supposed to give rise to various nitrides, each of which is capable, under certain conditions, of disengaging ammonia.

MEMOIRS ON THE VOLCANOS OF FRANCE, ITALY, AND SICILY.

1. Letters to Professor Jameson on the Volcanos of Auvergne. Ed. Ph. Journ., 1820—21.

2. Sketch of the Geology of Sicily. Do., 1825.

3. On the Diluvial Theory, and the Formation of the Valleys in Auvergne. Ed. Ph. Trans., 1831.

4. Note on a Paper of Dr. John Davy. Phil. Transactions, 1833.

5. On Lake Amsanctus and Mount Vullur in Apulia. Transactions of the Ashmolean Society, 1835.

6. Reply to Professor Bischof's Objections to the Chemical Theory of Volcanos. Ed. new Ph. Journal for April, 1839.

7. On the Site of the ancient City of the Aurunci, on Rocca Monfina, and on the Volcanic Phenomena there exhibited. Trans. of the Ashmolean Society, 1848.

These being for the most part embodied in my Descriptions of Volcanos, 2nd edition, 1848, (Richard and John Taylor); or in the Supplement to the same, 1858, to be had of the same Publishers; are therefore omitted in the present volume.

The two following are inserted, as having been published after the appearance of the last edition of my treatise on Volcanos.

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1. On the Antiquity of the Volcanos of Auvergne.

(*From the Quarterly Journa[illegible], Science, No. X. for April, 1866.*)

ACCUSTOMED, as we are, from our earliest infancy to have the first elementary truths of astronomy instilled into our minds, we can scarcely realize the idea, that nations in an advanced state of mental progress, were in the habit

of viewing our relations to the celestial bodies around us in quite a different light from that in which they present themselves to us at the present time.

Almost every child who has had a few months' instruction in a parish school knows, that the earth turns round the sun, that the circle which bounds our horizon is not the limit of the universe, that the moon revolves in an orbit which, although unapproachable by us from its distance alone, is near in comparison with the space which divides us from the sun, and yet that this great luminary is placed, as it were, within our own immediate neighbourhood, as compared with the distance which separates us from even the nearest of the fixed stars.

But these persuasions have grown up within a very recent period, and are due to the slow infiltration of philosophic truths into the minds of the vulgar, gradually displacing the earlier notions which had been acquired through the apparent testimony of our senses.

In the most flourishing periods of ancient Greece and Rome, in mediæval times, and even in ages approaching to our own, the same belief did not exist; nor, indeed, was a knowledge on such subjects a part of what the Almighty thought fit to impart by supernatural means to His chosen people.

Now, when it required so many centuries of research to indoctrinate the public mind in more enlarged ideas as to space, it cannot be a matter of surprise that geology, a science of much more recent date than astronomy, should not yet have succeeded in instilling more correct notions as to the extent of past time.

Without going back to times when this science was so much in its infancy, and was so little listened to out of doors, that the great body of the laity, as well as of the clergy, imagined the earth, as well as all the other celestial bodies, to have been called into existence by the direct *fiat* of the Almighty within the space of six literal days; and when it was taken for granted that the period which had elapsed since the creation of the universe was comprehended within the 6,000 years which, according to Archbishop

Usher's calculations, had elapsed since the birth of Adam, I can myself recollect when geologists of reputation, whilst contending that the days of creation must have embraced an extended duration, rather than a compass merely of twenty-four hours, took it for granted, nevertheless, that the latest epoch in the history of our planet—namely, that during which the climate and configuration of the earth's surface corresponded in its general features with those it exhibits at present—was ushered in by the appearance of man upon the globe, and consequently could not be traced back to an earlier date than that which on Scripture authority had been assigned for the first introduction of our species.

Upon the subject of man's antiquity I shall not enter; but with regard to that of the earth itself I may remark, that subsequent investigations have compelled us to enlarge very materially the allowance of time formerly allotted for its formation. They have shewn us at least that if what is called the post-pleiocene epoch is to be estimated as dating its commencement from the setting in of that intense cold which characterized what is called the glacial period, if to a temperature such as allowed of the growth of sub-tropical plants had succeeded, in the same latitudes as those of our own island, one as rigorous as that of Labrador at present, and if afterwards a gradual change supervened, by which the climate came by degrees to be assimilated to what we now experience, a longer interval must be supposed, than our received systems of chronology, built upon the assumption that man was a denizen of the earth throughout the whole of that series of events, would allow us to recognise.

And yet the time taken up in this the latest of the world's stages of progress, if I may so express myself, may bear no larger proportion to that occupied by the whole succession of formations from the first dawn of organic life upon the globe to the present time, than the distance in space between us and the moon bears to that which intervenes between our planet and the sun; just as even the time taken up by the deposition of all the rock formations, collectively considered, shrinks as much into insignificance by the side

of that required, according to the computation of mathematicians, for the cooling down of the earth from its original vaporous or incandescent condition, to a temperature such as admitted of the existence of life, as the distance between ourselves and the sun does to that which divides this luminary from the nearest of the fixed stars.

Thus, let us call the circumference of the globe 1.	And calling the distance from the present time to the birth of Adam 1.
Then the distance from the earth to the moon will be 10.	The distance from the same to the commencement of the post-pleiocene epoch would be 10.
Distance from the earth to the sun 400.	Distance in time from the commencement of the post-pleiocene epoch to that at which organic life began would be 400.
Distance from the sun to the nearest of the fixed stars (*viz.* 61 Cygni) will be 160,000.	Distance in time from the commencement of organic life to the period when the earth was first created, would be 160,000.

I have been led to these general remarks by the subject which has been proposed for the present communication, in which it will be attempted to shew that the phenomena presented by the extinct volcanos of Auvergne tend in a very marked manner to corroborate the inferences which, on other grounds, I have deduced with regard to the long space of time that must have been consumed even by that one stage in the earth's history which connects itself most nearly with the present, not to speak of that almost interminable series of antecedent deposits which contribute to make up the entire crust of the globe.

In order to render this subject more intelligible, it will be necessary for me to enter into some details, which may appear to some rather egotistical, as they will involve an account of my earliest visit to Auvergne, which took place in 1819, before any other British geologist, since the peace with France, had explored the district.

I had come at that time fresh from the lecture-room of

Professor Jameson, of Edinburgh, who was regarded a great authority in Geology, partly from the accurate knowledge he possessed of the characteristics of rocks and minerals, and partly as being one of the very few of our countrymen who had studied under Werner, the great Freyburg Professor, whose opinions respecting the structure and formation of the globe gave the law at that time to all who had studied under him.

Although in the lore which he had imported from his German master there was no small admixture of hypothesis, and that, as we now conceive, of a very crude and gratuitous character, yet the Professor contrived to impress his pupils with a high idea of the soundness of his instructions, not only for the reasons already assigned, but also from his dry and didactic manner, which seemed to preclude the notion of anything like fancy or imagination intermingling in the circle of his ideas. Coming forward, indeed, as the British representative of the Wernerian School of Geology, he felt it incumbent upon him in his lectures to exhibit the greatest possible antagonism to the treatises of Hutton, Playfair, and others of his countrymen who had appealed to Vulcan and Pluto as the main Artificers in the formation of the globe. With this view he founded, in opposition to the Royal Society of Edinburgh, where the Huttonian theory maintained its ascendancy, a new one, consisting chiefly of his own friends and adherents, which was denominated the Wernerian; and in order to render more patent the contrast between his mode of teaching and that of his opponents, he adopted the term Geognosy instead of that of Geology, by way of implying that his views were based upon observation, whilst theirs had drawn largely from the regions of imagination.

Now Werner had carried the Neptunian theory, as it was called, to such an extent, as to regard as deposited from aqueous solution, not only granite, but even basalt and traps of every form and description; and inasmuch as in Saxony, from whence his observations were chiefly derived, the trappean rocks occur in vast tabular masses overlying the other strata, he imagined the former to have been deposited in

consequence of a great inroad over the land, of water, carrying with it in solution the materials of which these rocks consist, so that the retiring flood left behind it on the summits of the highest ground it had reached those great deposits of trap which are found at this elevation.

And Werner's disciple, Professor Jameson, so far conformed to the creed of his master, that he stoutly maintained the aqueous origin also of all those formations of trap and porphyry which assume such gigantic proportions in various parts of Scotland, in the Hebrides, and in the north of Ireland.

In spite of the striking contrast which these rocks in their lithological characters present to ordinary deposits from water; in spite of the resemblance they bear to the products of fire; in spite of their intrusion into other strata in a manner which conveyed the idea of their former liquidity; and in spite even of the changes they often appear to have wrought upon the beds in contact, indicative, as it would seem, of fusion, Professor Jameson persuaded his pupils that the Wernerian theory was to be extended to them as well as to the rest.

But Auvergne, a region which had been already explored by Von Buch, then a young and rising geologist of the school of Werner, exhibited phenomena which seemed to many of us scarcely reconcilable with this conclusion; for whilst we were told of rocks existing in that district which were scarcely distinguishable from the traps of Scotland, we learnt on the other hand, that these same formations were found in intimate connection with craters of extinct volcanos and with the scoriæ ejected from them, forming even a part of the streams of lava which had descended from these igneous vents.

In undertaking, then, a journey through Auvergne, one especial object I had in view was, to see how far I could reconcile the Wernerian doctrine, which had been instilled into me by my late preceptor, with the facts that had come to my knowledge with reference to this particular district.

But although I started on my expedition just after I had been sitting at the feet of my Scotch Gamaliel, I had also

in previous years derived instruction in geology from quite a different kind of teacher, having attended the lectures of Professor Buckland at Oxford, which, although not professedly antagonistic to those of Jameson, exhibited the subject under quite a different aspect, both from his mode of treating it, and from the opposite character of his mental constitution.

Whilst Professor Jameson confined himself for the most part to a description of the older rocks, and considered them chiefly with reference to their lithological characters, Professor Buckland drew his illustrations chiefly from the modern, as his main interest lay in tracing the successive revolutions which the earth had undergone, as determined by the changes in organic life revealed to us by their petrifactions, as well as by the erosion of valleys, the transportation of erratic blocks, and the dispersion of gravel over the low ground.

Amongst the many catastrophes which he so vividly depicted, the latest, according to his reckoning, was that Deluge which Holy Writ had recorded, and which, instead of being confined (as many divines at the present day are content to regard it) to those regions which were actually peopled by man, had, as he conceived, left traces of itself in every part of the globe.

Those only who, like myself, can recollect the early lectures he delivered in the old Ashmolean building, to which Academics of all degrees of standing, from the Freshman to the Head of a House, flocked from every College and Hall of the University, can form an idea of the interest he inspired in this new study, by that union of vivid description, extensive knowledge of details, boldness of speculation, drollery, and enthusiasm with which he fascinated the minds of his hearers.

I may observe, however, that not only in those lectures of his which I attended in the years 1815 and 1816, but long subsequently, as in his *Reliquiæ Diluvianæ*, published in 1824, the Professor appealed to the organic remains contained in caves, fissures, and diluvial gravel, and to other phenomena, as attesting the action of an universal deluge.

And such was the influence upon the scientific world which

he commanded by the zeal, eloquence, and research which he displayed in carrying out his favourite hypothesis, that diluvial action, diluvial gravel, diluvial pebbles, diluvial detritus, and the like were for many years received amongst the household words of geologists both in England and on the Continent.

It is true, that the scientific evidence by which he supported the notion of an universal deluge broke down upon further inquiry, and thus an early lesson was afforded us of the imprudence of pressing hastily into the defence of religion even the most plausible inferences deducible from the facts of science. The risk of thus prejudicing the cause which it was intended to uphold, by the re-action produced upon the mind from the subsequent exposure of the fallacies involved in the argument advanced, has been lately pointed out by Dr. Pusey in his discourse on "The Relation of Science to Religion," delivered at the Norwich Church Congress, and will, I believe, meet with general assent amongst men of science as well as theologians of all parties.

Still at the time I first visited Auvergne, the position that the valleys had been excavated by the action of the Noachian deluge reigned undisputed; and thus I began my explorations in that district with a mind prepossessed, not only with the doctrine of the aqueous origin of trap which I had derived from my Edinburgh preceptor, but also with the idea that the valleys in every country were the results of this supposed catastrophe.

It did not require more than a few days' sojourn in Auvergne to disabuse my mind of the former opinion, for the association of rocks undistinguishable from the traps and porphyries of Scotland, with craters, lava streams, and heaps of scoriæ, which plainly attested the operation of volcanic heat, soon convinced me that the Wernerian doctrine as regarded the origin of basalt was untenable.

But the theoretical views which I imbibed from my Oxford instructor were not so easily got rid of; for the phenomena presented by the volcanos of Auvergne proved of a nature to afford an apparent confirmation of the distinction which Dr. Buckland had set up between rocks formed

before and *after* the Noachian deluge, or, according to his nomenclature, those of ante-diluvial and post-diluvial origin.

The volcanic rocks which we observe in this part of Central France may be separated into two classes, both by the difference in their external characters, and also by their position with reference to the surrounding strata.

The first class consists of those which have been cut through by the existing valleys like the other rocks of the district; the second, of those which follow the inequalities of the surface, so as to prove that they were ejected since the country had acquired its present configuration in all its important features.

Subsequent observations have, indeed, shewn that no sharp line of demarcation exists between the two, as there are instances of volcanic rocks which, although they have descended the slope of a valley, were themselves scooped out to a great depth by the same agency. But that the distinction can be clearly traced in many instances cannot, I think, be disputed by any who have visited the locality.

Moreover, a great difference exists between the two classes in their external characters, the former possessing in general the compactness and stony aspect belonging to trap and porphyry, the latter that cellular glassy appearance, and harsh feel which characterize modern lavas.

The former accordingly support a luxuriant vegetation, whilst the latter are scarcely decomposed by time, and therefore afford but little pasturage, and that generally of the worst description. The former, too, constitute extensive sheets of rock spreading over large districts, and cannot be traced to any point of issue, whereas the latter may generally be found to emanate from a crater, from which they proceed into the lowest ground contiguous, in a stream, the breadth of which bears no proportion to its length.

In accordance, then, with the views prevalent at the time I first visited the country, the former class would be entitled to the name of ante-diluvial, and the latter to that of post-diluvial, the one being regarded as produced before the great catastrophe by which the valleys of the country were then conceived to have been excavated, the latter subsequently to that event.

It therefore became a question of some interest to determine, whether any records existed which should indicate the continuance of the activity of the Auvergne volcanos down to the period of authentic history. But on this point classical authorities are silent.

Julius Cæsar, by no means inattentive to the external features of the countries he invaded, makes no allusion to any volcanic phenomenon having arrested his notice, although he encamped upon the plains of Auvergne, and laid siege to Gergovia, the principal city in the district.

His silence, however, it may be said, only proves that during the period at which he was engaged in that part of Gaul the igneous forces were slumbering, as might have happened in the case of Etna or Vesuvius during any such short interval of time.

But what shall we say of the omission of Pliny to include Auvergne amongst the regions in which he records the existence of fiery mountains; and what of the silence of Strabo on the same point? to say nothing of the poets, who indulge in such frequent mention of Etna and Lipari, but make no allusion to volcanos in other portions of the then known globe.

And yet of all natural phenomena a burning mountain is the one which in all ages most forcibly rivets the attention of the multitude, and of which the memory is longest retained by tradition, or by means of the popular fables engrafted upon it.

The volcanic fire once raging near Lemnos, about Santorino, in Argolis, and elsewhere, is made the subject of many a poetic legend, and even less formidable phenomena of the same kind, such as the bursting forth of flames from the ground, are carefully recorded by the naturalists of Greece and Rome.

The most convincing proof, however, that the volcanos of Central France were not in activity so late at least as the fourth century of the Christian era, or at any period antecedent to this, which would be included within the range comprised by the histories or the traditions of the country, is afforded by the absence of all allusion to such phenomena in the existing works of Sidonius Apollinaris.

We learn from some of his writings which have come down to us, that he had a palace on the borders of Lake Aidat, in the very midst of the volcanic region alluded to. In one of his poems he celebrates the beauties of this residence, and compares it to his former abode at Baiæ, near Naples; but not a hint escapes him that he had ever witnessed or even heard of any volcanic eruption in his neighbourhood, although he must have been familiar with the nature of such phenomena, from having previously resided in the neighbourhood of Vesuvius.

And yet the very lake near which his episcopal palace stood, owed its origin to one of the more recent, or, according to Dr. Buckland's hypothesis, of the post-diluvial eruptions, one which, invading the bed of the little river Sioule, by the stream of lava it sent out, raised a barrier across it, and ponded up its waters, until they accumulated to such an extent as to form a considerable sheet of water.

It was on these grounds that in the account I published in 1819 of this my earliest visit to Auvergne, I concluded that although some of the volcanos of this country might have been in activity since the epoch of the Noachian deluge, they must all have been extinct before the Roman invasion; and this conclusion was assented to by Mr. Scrope, Sir Charles Lyell, and other geologists, who afterwards explored the district.

Nevertheless, in the year 1844, a different view of the subject was advanced by an eminent historian and antiquary, Sir Francis Palgrave, who, in an article "On the Norman Conquest," in the "Quarterly Review," endeavoured to shew by some quotations from the writings of Sidonius Apollinaris, and of Alcimus Avitus, Bishop of Vienne, that the volcanos of Auvergne had been in activity so late as the fourth century after Christ [a].

In reply to the arguments he advanced, I observed [b], that although some of the expressions of Sidonius and of Alcimus

[a] Reference was made to *Sidonii Apollinaris,* Epist. i.; and Alcimi Aviti *Homiliæ de Rogationibus.*

[b] See my work, entitled "Descriptions of Volcanos." (Taylor and Francis,) 2nd ed., 1848, p. 31.

Avitus quoted might seem at first sight to refer to a volcanic eruption, yet the following considerations would forbid of our entertaining such an hypothesis.

1. The city of Vienne, near which the physical convulsions alluded to were reported to have occurred, lies nearly seventy miles in a direct line from the theatre of volcanic action situated round Clermont.

2. Had Sidonius ever heard of such events occurring in his own neighbourhood, he would scarcely have failed to refer to them, if not on other occasions, at least on this, in which he vividly depicts the alarming catastrophes that had occurred in a neighbouring province.

3. The city of Vienne stands upon gneiss, with the great coal formation of St. Etienne interposed between it and the rocks of Auvergne, and with a range of hills of considerable elevation intervening between the two.

4. The geology of France has been carefully explored by the labours of Elie de Beaumont and others; but no notice of anything volcanic in the vicinity of Vienne can anywhere be found in their descriptions.

5. No volcano could have burst out without leaving permanent traces of its occurrence by the craters and lava-streams which it must have produced.

6. It is contrary to the analogy of other active volcanos, to suppose that an eruption should have broken out at such a distance from the sea as that at which the city of Vienne is situated.

7. Allowance being made for some little exaggeration on the part of the narrators, the descriptions both of Sidonius and of Avitus might apply to those dreadful earthquakes, which, as we have reason to believe from the subsequent testimony of Gregory of Tours, ravaged the whole of this district, and which may easily be supposed to have extended from the former seat of volcanic activity, Auvergne, to a neighbouring province, just as the most fearful earthquakes in Sicily are experienced, not at Catania, but at spots as far distant from Etna as Palermo.

The underground thunder, the opening of fissures in the ground, the bursting out of flames and gases, the projection

of water and of stones, the smell of sulphur, the alarm evinced by the animals of the spot and neighbourhood, the elevation or depression of the land, noticed by Sidonius and by Avitus in the passages referred to by Sir Francis Palgrave, are all reported as concomitants of the great earthquakes which have occurred in more recent times.

On the above grounds I continued sceptical as to the late date assigned to the volcanos of Central France, even after weighing the arguments which Sir Francis Palgrave had brought forward in support of his opinion; and as this distinguished writer never appears to have replied to my arguments, (although in his "History of Normandy," published in 1856, he briefly reiterates his statement, without, however, remarking upon the grounds which had led him to adhere to it,) I considered the question to be set at rest, until last year, when the controversy between Bishop Colenso and his antagonists, relating to the accuracy of the historical portions of the Pentateuch, unexpectedly led to its revival [c].

The Bishop, it seems, contended, that if the deluge recorded in Genesis had been universal, it must have swept away those cones of loose scoriæ which are found in many parts of Auvergne, the great antiquity of which he inferred, amongst other reasons, from the admitted fact, that all volcanic action had ceased in the country before history commenced.

To this his opponents replied, by appealing to the evidence already got together by Sir Francis Palgrave, in proof that such operations had continued there as late as the fourth century after Christ.

Now, with reference to the question at issue between the Bishop of Natal and his opponents, it seems to me to matter little which side of the controversy is espoused, and therefore the attempt to give a theological aspect to the discussion, strikes me as wholly uncalled for. For, on the one

[c] The controversy on this subject between Archdeacons Philpotts and Garbett on the one hand, and Bishop Colenso on the other, together with some letters of my own touching the same question, was published in the "Guardian" newspaper.

hand, supposing it to be established that the volcanos of Auvergne had continued active as late as Sir Francis Palgrave imagined, it would still remain to be ascertained whether those particular outbreaks to which Colenso had appealed could be referred to a date subsequent to the Noachian deluge; and on the other, supposing it probable that all traces of igneous operations had ceased before the earliest period to which history points, there would still be an ample margin left between that and the supposed date of the Flood, to allow of these outbreaks having taken place.

Let us, then, enter upon the inquiry with minds unswayed by any bias, and simply consider whether it be probable that, in the passages above alluded to, anything of the nature of a volcanic eruption could have been intended.

And for my own part, as no new arguments were advanced in support of those alleged by Sir Francis Palgrave, I feel still at liberty to adhere to the opinion which had been taken up long before its possible bearing upon any polemical question was dreamt of, and to maintain, as I did in the year 1819, that the volcanos of Central France have not been shewn, by evidence yet adduced, to have been in activity at any period within the range of history or tradition.

And now, having, as I hope, disposed of this previous question, let us proceed to consider whether that class of volcanos which I denominated *post-diluvial*, but which I shall now merely designate as, by comparison, *modern*, presents any characters indicating great antiquity.

In fixing their age, I have derived great assistance from the researches of those eminent geologists who, since the period of my first visit to Auvergne, had explored the district in question, and especially from those of Mr. Scrope, who appears to have spent there the summer of 1821, and those also of Sir Roderick Murchison and Sir Charles Lyell, who went through the country in 1828.

From the descriptions given by these and other competent authorities, it plainly appears, that the valleys in Auvergne were excavated, not at one, but at several successive periods

—or, more correctly speaking, that although water was instrumental in their formation, yet that they must have been scooped out, not by any violent movement or sudden passage of a flood over the country, but by the long-continued action of the rivers now in existence.

And if this be the case, it follows, that there can be no abrupt line of demarcation between the older and the more modern volcanic products, and that even those which have been ejected since the formation of the principal valleys, may nevertheless afford evidence of extreme antiquity.

It is but fair to attribute to Mr. Scrope our first correct notions on this subject.

His "Memoir on the Geology of Central France," published in 1827, evinces a just idea of the mode in which its valleys were formed, as well as a clear appreciation of the amount of time which must have been occupied in their excavation, and his work is illustrated by a number of interesting panoramic views, which bring vividly before us the general physiognomy of the country, so as better to enable us to realize the force of the evidence he had brought forward.

To this work, and to the memoir of Lyell and Murchison, "On the Excavation of Valleys, as illustrated by the Volcanic Rocks of Central France," I am chiefly indebted for the few facts the space allotted me admits of my bringing forward, in proof of the great antiquity even of the more modern class of eruptions.

Let us take the case of the volcano of Chaluzet, near the village of Pont Gibaud.

This is a conical hill, composed of red and black scoriæ, having on its summit a depression resembling a worn-down crater, from which may be traced a powerful stream of lava descending into the valley below, in which the river Sioule flows. Deflected to the north-west by the lofty and serrated ridge of gneiss which forms the right bank of the stream, the lava-current follows its course as far as "Les Combres," where it terminates.

The upper portion of the mass is scoriaceous, the lower compact and prismatic, and the under surface of the prisms

stands at a height of about fifty feet above the present bed of the Sioule, resting upon a bed of pebbles.

The pebbles have indeed been traced some way into the rock, in consequence of a gallery driven in horizontally through the upper part of the gneiss and the interposed alluvium, so as to render it clear that the lava stream really rests upon the latter.

Hence it follows, that since the period at which the lava was ejected, a thickness of fifty feet of solid gneiss must have been excavated.

Now the slowness with which the present river erodes a material of this description may be estimated by a fact pointed out by Sir Charles Lyell in the same province, near St. Nectaire, where an ancient Roman bridge spans the river Couze, over a stream of lava, proceeding from a volcanic hill,—the Puy de Tartaret,—shewing that a ravine, precisely like that now existing, had already been excavated by the river fourteen centuries ago.

And yet the lava of the Puy de Tartaret presents all the appearance of a modern current, both from its having conformed to the sinuosities of the valley, and also from its covering a bone deposit at its bottom, indicating a mammiferous Fauna, which, although distinct, as a whole, from that now inhabiting Auvergne, presents some features in common with it, as in the existence of the dog, deer, cat, &c., mixed with the remains of the reindeer, which, even so late as the time of Cæsar, appears to have been found in the Great Hercynian Forest, and also with an animal of the horse tribe, differing, however, in some points from the species now living.

But it is in the neighbouring province of the Vivarais, that the most remarkable instances of the long-continued action of water slowly eroding to a great depth streams of lava which have flowed at a comparatively recent date, are afforded.

Before describing these, however, I must point out a circumstance which distinguishes a current of lava from one of water, namely, that from its viscid character it has a tendency, near its termination, to accumulate layer upon layer,

so that its materials are piled up to a considerable height, instead of spreading onwards, as would happen to a substance of more perfect fluidity.

Hence, when a lava stream reached the bed of a river, it sometimes formed a precipitous bank on one side of it, without appearing to have advanced to the other.

Of this, indeed, several examples are met with in Auvergne, but the most remarkable cases are those to which I have alluded in the Vivarais.

In that province, Mr. Scrope enumerates no less than six perfect volcanic cones, with craters on their summits still preserved in a state of greater or lesser integrity, from which have proceeded streams of lava, each traceable down the sides of the mountain, and seen to terminate abruptly at its foot.

Now, when the bottom of the valley is occupied by running water, its bank is walled in by a colonnade of basalt, extending for a considerable distance along its margin, derived from the lava stream which had descended from the mountain above.

It is true that when, as sometimes happens, the igneous mass is not perceived on the opposite bank of the river, as is the case at the Coupe de Col d'Aisac, of which a description and drawing has been presented us by Faujas St. Fond, we have no right, for the reasons above stated, to ascribe the entire height of the vertical cliff of basalt to the eroding force of water; but, in other cases, as at the spot called the Gravenne de Montpezat, of which Mr. Scrope has given us a drawing, there can be no mistake about the matter, as a high, precipitous rock, upon which the ruins of a castle stand, is severed from the main body of the lava current, and rises up in the midst of the stream. The upper portion of this rock is composed of basaltic lava, derived from the mountain above, and forming the termination of a current which had flowed from it; but the lower consists of gneiss, which, since the lava current had been erupted, is seen to have been excavated by the erosive power of the stream to the depth of a hundred feet. The time necessary to bring about this effect I will not pretend to estimate, but may

appeal to it as a proof of the great antiquity of a lava current, which must have, at least, been antecedent to the commencement of the operation by which it was occasioned.

One very remarkable peculiarity of the lava streams in the Vivarais currents is their basaltic character and their prismatic structure. We are accustomed to consider trap rocks in general, and more especially that particular description which is denominated basalt, as exclusively the product of submarine volcanos, their compactness being said to arise from the great pressure exercised upon them during their consolidation. But in this part of France we meet with several instances of basaltic colonnades, which have been evidently derived from streams of lava ejected from subaërial volcanos.

It is true, that, in all those specimens which have come under my notice, minute cells and cavities may be discovered by careful examination, and, moreover, that the upper portions of the bed are more pervaded by them than the lower.

Still the resemblance which they bear to the products of submarine volcanos is very remarkable, and only admits of being explained by the thickness of the bed and the weight of the scoriæ superimposed, for it evidently matters not in what way the pressure is produced, provided it be sufficient to retain the aqueous and other volatilizable ingredients present within the rock in such a condition, as to prevent the production of cells and cavities.

And, accordingly, it is observed, that this compact character and columnar structure are not met with in those parts of the current which occupy the slope of the mountain, but only at its termination in the valley below, and that even there these characters are confined to its lower portions, where the pressure must have been greatest, the basalt being surmounted by a considerable thickness of cellular lava of the usual kind. Moreover, a difference can be traced in the degree of its compactness, according to the relative position which the specimen holds in the basaltic bed, the upper layers being the most cellular.

In the Vivarais, then, as well as in Auvergne, we have

abundant instances of lava streams, which, although amongst the most recent the district affords, being poured forth at a time when the general configuration of the country had become nearly what it is at present, exhibit, nevertheless, traces of their high antiquity, from having been subjected to the long-continued operation of denuding agents.

Where these agents have been at work their relative date may be fixed, but we do not appear to possess the same means of referring to a particular epoch the five isolated domes of trachyte which occur on the table-land to the west of the city of Clermont, although the occurrence of free muriatic acid in one of them would imply that they were modern.

These conical hills, of which the loftiest, called the Puy de Dôme, rises to the height of 4,842 feet, or 3,554 feet above the level of the town of Clermont, seem each to have proceeded out of the midst of a kind of crater formed by volcanic rocks of the usual character and appearance, and therefore bearing no analogy to the material of which they are themselves principally constituted.

They seem to shew some resemblance, although they are on a much larger scale, to the Bosses or *Mamelons,* to use a French phrase, protruding from the midst of the craters of Rocca Monfina, near Terracina, and of Astroni, near Naples, which may perhaps be paralleled by those dark spots observed by astronomers in the midst of the circular hollows existing on the surface of the moon, which Sir John Herschel and others have regarded as volcanic craters. Without discussing the mode of their formation, which would detain us too long, it may be enough to say that they would seem to be more modern than the amphitheatre of volcanic rocks which encompasses them, though their elevation, which places them beyond the reach of the eroding action of rivers, prevents our fixing with any certainty the degree of their antiquity.

Let us therefore pass from these problematical rocks to others of far greater antiquity than any that have yet come before us.

They may be divided into two classes—namely, those of a basaltic and of a trachytic character, and of these the latter seem in general to lie lowest, having the basaltic superimposed.

But since the trachyte at the same time rises to the most elevated points in the country, as at the Pic de Sancy, near the Baths at Mont Dor, where it attains the height of 6,217 feet above the sea, and in the neighbouring department of Cantal, where it reaches, at the summit of the Plomb de Cantal, that of 6,096, the basalt seems in some places to lie beneath it.

Although volcanic, these formations bear but a remote resemblance to the rocks previously alluded to; for not only are the materials of which they are composed in general more compact, but when scoriform, they consist for the most part of pumice, a material not met with, it is believed, amongst the more recent class of volcanos.

Still more distinct, too, is their general structure; for, instead of constituting streams of lava traceable for the most part to a crater as their point of issue, they are spread out into vast sheets, extending continuously over wide areas, in some places indeed rising to a great elevation, but even then exhibiting no traces of anything which bears the slightest resemblance to a crater.

Indeed, so contrasted are the general characters of the volcanic rocks we are considering, with those in the neighbourhood of Clermont, that Messrs. Dufrenoy and Elie de Beaumont, the French geologists alluded to, conceived that their structure may be best explained upon the supposition, that they had been first spread horizontally over the surface of the subjacent gravel, and afterwards were upheaved at three different points, the Pic de Sancy being the centre of one elevatory movement, Roche Sanadoire of a second, and the Puy de la Tache of a third, these representing the highest spots in the vicinity of the Baths of Mont Dor, around which the supposed elevatory movement had taken place.

Now the deep valley in which these Baths lie is conceived by these same geologists to have been originally formed, not by the erosion of water, but by a disruption of the rocks on

either side, consequent upon the elevation of the range at these three several points.

If this theory be adopted, we are precluded, of course, in this instance, from appealing to the great depth of the valley, at the bottom of which the Baths stand, as indicative of the time required for eating so deeply into the substance of the volcanic rocks which bound it; but other proofs of great antiquity are not wanting, such as the existence of conglomerates consisting of rolled pebbles, which underlie one volcanic bed and which support another, as well as of tuffs containing fragments of the trachyte and basalt of the neighbourhood.

In some places, also, as in the department of Cantal, fragments of limestone containing impressions of plants are scattered through these trachytic conglomerates.

And it would be a bold thing to maintain that, whatever may have been the case with the particular valley alluded to, none of the others which score the sides of the volcanic table-land have been due to the action of water, or even that such as have been originally produced by upheavement were not subsequently modified by denuding agents.

In short, the same arguments which induce geologists to assign a very long duration to those operations of nature which have in other countries scooped out the valleys, and moulded into its present form the earth's surface, apply equally to the case of that more ancient volcanic region in Central France which has been just alluded to.

Everything therefore concurs to bespeak a high antiquity for these formations, and to indicate a long-continued operation of denuding forces upon the beds of igneous matter since their eruption; and yet all these events must have been posterior to the formation of some at least of the fresh-water beds of the Auvergne country, formations which Sir Charles Lyell refers to the Eocene period, still a portion of the Tertiary or of the youngest member of the great family of rocks.

It seems indeed most probable, that these eruptions of igneous matter had broken out at the time when the dis-

trict was covered by extensive sheets of fresh water, like the great lakes of North America, and hence may have been derived their greater compactness, as compared with the more modern volcanic products before alluded to, an indication of their having been erupted under a pressure greater than that of the atmosphere. And yet, when we recollect that in the Eocene period about $3\frac{1}{2}$ per cent. of existing species of molluscæ were already in being, whereas in the newest of the subjacent secondary rocks no one living form has been as yet detected, and when we consider, moreover, how many distinct races of animals and of plants, all of which have passed away, succeeded each other in periods antecedent to the first dawn of the Tertiary epoch, it must be admitted, that vast as was the time occupied in bringing about the long series of igneous formations which we witness in Auvergne, it sinks almost into insignificance by the side of that period of incalculable duration, which must have elapsed since the globe became first fitted for the maintenance of organic life.

And this leads me to another point of some general interest—namely, that volcanic action, notwithstanding its long continuance in one district, shifts its ground from time to time, so as probably in the course of years to visit in succession every region of the globe.

Before the close of the Eocene period, when the volcanos of Auvergne first came into activity, those of the Hebrides, of the North of Ireland, and of parts of Scotland, had become extinct, and yet we have reason to believe that these last were for the most part contemporaneous with the chalk, and do not date back so far as the Oolite.

That they are entirely *burnt out*, may be inferred from the absence, throughout the whole space comprised within their several areas, of thermal springs, and of the severer forms at least of earthquake; which cannot be said of Auvergne, for the latter volcanos, though, as I believe, not in activity at the earliest periods of history, still give evidence of smouldering internal fire, in their warm springs, evolutions of carbonic acid gas, and occasionally recurring earthquakes of considerable intensity.

It would be easy to point out volcanic regions of still greater antiquity in other parts of the globe, which became extinct even before the igneous operations in the Hebrides, &c., had commenced; but it may be most to the purpose to note, that in the highly vulcanized region of Southern Italy, the Apennine limestone, there so abundant, and of an age corresponding to the Jura or Oolite, exhibits no proof of igneous action having extended back so far as the period at which their beds were deposited.

From these considerations it may be inferred, that every portion of the globe is destined at one time or another to become the theatre of similar catastrophes.

Perhaps at some future period a chain of burning mountains may shew itself along the coasts of Scandinavia; perhaps Australia may hereafter experience some of those underground convulsions which are now so rife amongst the islands of the Pacific.

And if so, what an impression is conveyed to the mind, as to the length of time which must have elapsed, since the planet we inhabit was first called into existence, or, indeed, even as to the number of years which have rolled on since the commencement of organic life.

For from the duration of only one of these epochs—that which has been pointed out in Auvergne, we may form some slight estimate of the remainder, and the aggregate certainly presents an idea of past time which it is difficult for our limited faculties fully to realize.

On the Ignigenous Rocks near Montbrison.

(WITH REFERENCE TO THE ANTIQUITY OF THE VOLCANOS OF CENTRAL FRANCE.)

(*From the Quarterly Journal of Science, No. XIII. for January 1867.*)

IN the April number of the "Quarterly Journal of Science" for 1866 will be fouud a memoir of mine, "On the Antiquity of the Volcanos of Auvergne," in which, in opposition to the late Sir Francis Palgrave, and to certain divines who had followed in his footsteps and adopted his views, it was attempted to shew, that even the latest of the eruptions proceeding from these mountains must date from a period antecedent both to history and tradition.

But as it must at the same time be conceded, on the testimony of two bishops whose writings have come down to us, namely Sidonius Apollinaris and Alcimus Avitus, that during the fourth century after Christ, certain physical commotions took place in the neighbourhood of Vienne in France, which were of a nature sufficiently formidable to suggest the offering up of public prayers, and even the institution of the Rogation-days, set apart ever since in the Church for divine worship, those who denied the recent date of the volcanic eruptions in that neighbourhood were called upon to shew, that there are no vestiges of the kind round about the city of Vienne, which might by possibility be referred to a period comparatively so modern as the one alluded to.

I therefore pointed out in the above memoir, not only that, so far as is known, volcanos are entirely absent from the immediate vicinity of the city of Vienne, but also that the nearest indications of igneous action to be met with,

occur either about Issoire, in the neighbouring department of the Puy-de-Dôme, a town situated in a straight line at a distance from Vienne of about eighty English miles, or else near Puy-en-Velay, which is not less than sixty from the same locality.

It has, however, since been suggested to me, that I had overlooked a little group of volcanos situated round about Montbrison, the capital of the department of the Loire, a town which lies considerably nearer to Vienne than either of the places to which my attention had been directed, being in fact not more in a straight line than about thirty-five miles distant from the city of Vienne, and that it was possible, therefore, that the convulsions of nature to which Sidonius and Alcimus refer might find their explanation, in certain eruptions of which this neighbourhood had still retained the impress.

I was, therefore, glad to avail myself of the opportunity of visiting, in company with my friend Mr. Corfield, a Fellow of Pembroke College, Oxford, the above locality on our way to Switzerland this autumn, and I am now prepared to say, that, without pretending to have surveyed the entire district, I saw enough to convince me, that no volcanic disturbance which had occurred within this area at so late a period as that alluded to could have escaped our notice, and that every indication of igneous action which presents itself throughout the country bears marks of a much greater antiquity.

Thus much at least I can venture to affirm, namely, that neither craters, streams of lava, scoriæ, nor even cellular trap, are to be met with anywhere within the limits of this district. On the contrary, the only igneous rocks which came under our observation consisted of a compact basalt, containing nests of olivine, a material which could only have been elaborated by the aid of great pressure, and under a different configuration of the surface from that now existing.

To descend to particulars—the granitic formation, which occupies a large portion of central France, may be seen extending to the west of Montbrison, but the valley of the

Loire, in which this town is itself situated, consists of tertiary fresh-water beds, covered over in many places by thick deposits of alluvial matter.

On the right bank of the Loire, however, the granite is again seen, and stretches as far as the Rhone valley, in which Lyons is situated.

Again, farther to the south, occurs the Coal formation of St. Etienne, which consequently intervenes between the valley of the Loire, in which Montbrison stands, and the city of Vienne, situated on the banks of the Rhone, which also is built upon a granitic rock.

Now both to the north and south of Montbrison, is descried, elevated above the general level of the granitic formation, a number of isolated knolls rising abruptly to a height of 500 feet or upwards, and in general capped by the ruins either of a church, a convent, or a castle, for which these summits would have been especially well adapted, both as being conspicuous objects from a distance, and also as their steepness would render them secure from assault.

Having visited several of these little detached hills, as for instance, St. Romain-le-Puy to the south, and Marcilly-le-Pavé and Montverdun to the north of Montbrison, I can state, that each of them is composed up to its summit of basalt, which also extends nearly down to the level of the surrounding country.

At St. Romain-le-Puy, and Marcilly-le-Pavé, the trap rock rests upon granite, but that of Montverdun is incumbent directly upon the tertiary formation, which, as before stated, is superposed upon the granitic rocks on the lower levels.

Moreover, to the east of the road leading from Montverdun to Montbrison is a ridge, the longer axis of which lies nearly from north to south, wholly made up of the same material.

About half an English mile from Montbrison itself, at a place called "Le Roche," occurs the most instructive section which came under our notice; for here about half-way up the hill the basalt may be distinctly seen intruding itself into, and thrust through, the midst of the granite,

which is in consequence uplifted, and manifests itself both above and below the igneous rock, in the quarry, where the latter for road purposes is extensively worked.

Indeed the granite occupies a much more elevated position than this on the hills to the west of the spot where the basalt is seen, for the latter is found only at a certain elevation, being bounded both above and below by the granite of the country.

Judging from these facts, which are thoroughly borne out by the negative evidence, stated in the former part of this communication, I should conclude, that a vast antiquity must be assigned to the basalts which occur about Montbrison, for one can only account for the isolated position in which they are found on the detached knolls scattered over the district, by supposing that they constituted a part of one great continuous sheet of volcanic materials, which once overspread the surface, and of which the intervening portions have been since removed by denudation.

Of course such a supposition removes their origin to an immeasurable distance in point of time from any physical convulsions of recent or historical date, and indeed from the whole modern class of volcanos which has been described in my former memoirs on this country. They remind one of the basaltic eminences met with in Saxony, which Werner referred to his imaginary flœtz-trap formation, with reference to which also we are compelled to assume, that the detached knolls of basalt scattered over the country, and resting upon the sandstone rocks which there predominate, are remnants of some great overflow of molten materials which covered the country, when the now elevated peaks constituted its lowest level, and when in all probability the entire district lay submerged under the ocean. We are therefore only obliged to transfer to Vulcan the task which the renowned geologist of Freyburg attributed to Neptune, and to conceive that a flood of melted matter discharged from his subterranean workshop overspread the district, instead of the deluge of water charged with mineral matter which, according to Werner, had risen to the summit of the highest hills, and which had left behind it

on its retreat those flœtz-trap rocks which he persisted in referring to an aqueous origin.

It would appear, then, that the conclusion at which I arrived in my previous Memoir is in no respect invalidated by anything observable at or about Montbrison, and that we are still at a loss for any facts tending to shew, that the lively picture drawn by Sidonius "of the earthquakes which demolished the walls of Vienne, of the mountains opening and sending forth torrents of inflamed materials, and of the wild beasts, driven from the woods by terror and hunger, retreating into and making great ravages within the towns," is to be regarded in any other light than as the offspring of a lively imagination, dwelling upon reports which had reached the author, with respect to some fearful earthquake which may have occurred in the neighbourhood of Vienne.

MINERAL AND THERMAL WATERS.

On this subject I have published the following Essays and Memoirs:—

1. "On the Discharge of Nitrogen Gas from various warm Springs." (*Bibliothèque Universelle de Génève*, 1830 [a].)

2. "On the occurrence of Iodine and Bromine in certain Mineral Waters of South Britain." (Philosophical Transactions, 1830.) Noticed in Part I.

3. "Article on Mineral Waters." ("London Review," conducted by Blanco White, 1831.)

4. "Remarks on Thermal Springs and their Connexion with Volcanos." (Edinburgh Philosophical Journal, 1832 [b].)

5. "Remarks on a certain kind of Organic Matter found in Sulphureous Springs." (Linnæan Transactions, vol. xvi. for 1833.)

In this Paper the white or organic matter collected round some of the Thermal sulphureous Springs of the Pyrenees was described, and referred to the growth of a species of *Conferva*, probably allied to the *Conferva nevea* of Dillwyn [c]. This substance had been imagined by Anglada, Gimbernat and others to be a chemical product derived from the interior of the earth, but the observations detailed in this Memoir sufficiently evince the fallacy of that opinion.

6. "On the Quantity and Quality of the Gases disengaged from the Thermal Spring which supplies the King's Bath in the City of Bath." (Philosophical Transactions, 1834.)

7. "Report on the Present State of our Knowledge with respect to Mineral and Thermal Waters." (Reports of the Sixth Meeting of the British Association for the Advancement of Science, 1837.)

[a] The substance of this Paper is given in the List of Springs which evolve Nitrogen Gas, placed after the Memoir on the Thermal Waters of Bath.

[b] A brief account of these researches is given in Delabeche's "Geological Manual," 1833, p. 606.

[c] Dillwyn's "Confervæ."

8. "On the Thermal Waters of the United States." (From a Sketch of the Geology of North America, read before the Ashmolean Society of Oxford, and published in their Transactions for 1839.)

The substance of the above communications has been already, for the most part, incorporated in the thirty-fourth and thirty-fifth chapters of the second edition of my Description of Active and Extinct Volcanos, published in 1848, so that it seems unnecessary to reprint any of them, especially as a *resumé* of the general conclusions deduced from the facts detailed is given in a Paper read by me to the Chemical Section of the British Association at Bath in 1864.

As the latter Memoir conveys the most recent information I have to offer on the subject, and likewise contains a succinct account of the chemical theory, which professes to account for the phenomena both of volcanos and of thermal waters, I shall next subjoin it.

Memoir on the Thermal Waters of Bath.

Communicated to the Chemical Section of the British Association, held at Bath under the Presidentship of Sir Charles Lyell, in 1864.

OUR distinguished President, in his inaugural discourse, has entered so fully into the subject of Hot Springs, that it may appear unnecessary for me to detain the Section with any remarks of my own on such a subject.

Nevertheless, as some of the older inhabitants at least of Bath may recollect, that I was engaged for more than a month in 1832 in investigations connected with these thermal waters, and that the results I had arrived at were deemed worthy of a place in the Philosophical Transactions of that year, it will perhaps be expected, that I should take this opportunity of communicating to one of the Sections some of the facts and conclusions at which I had arrived at the period alluded to, since those who are not acquainted with my original Memoir may desire to have its principal contents briefly laid before them on this occasion; and those who are, may still be curious to learn whether my

own later and more matured experience, or the subsequent observations of others, have tended to modify or overthrow the theory which at that rather distant period I ventured to propound.

And, although the origin of thermal waters is a question which belongs more properly to the domain of Geological Science, as will indeed be appreciated more fully, after the masterly exposition of their nature which we have just received from a professed geologist; yet, as the only claim I have ever had to be considered one, is founded on my attempts to apply the principles of chemistry to the explanation of physical phenomena; and as the reasonings which I have to submit to you in the present paper will best be appreciated by chemists, I have deemed it most appropriate to offer these remarks on the thermal waters of this place to the Section which I have now the honour of addressing.

I should state, in the first place, that the investigations to which I allude have no reference to the medicinal virtues for which the Bath waters have so long been celebrated. These, which, in the eyes of most of my audience, naturally figure as the foremost of the questions which can come before us in connection with the thermal springs alluded to, must, I suspect, be assumed merely upon the testimony of the many persons who have from time to time experienced benefit from their use, as they could not have been deduced *à priori* from a knowledge of the properties belonging to their ascertained constituents, whether individually or collectively taken.

In this respect, however, Bath only stands upon the same footing with other thermal waters of established reputation, some of which even, such as the far-famed springs of Gastein, in Austria, are, so far as we know, entirely destitute of any mineral ingredient that could affect the system, so that in their case we seem only to have the alternative of concluding, that the benefit derived from their use arises from the change of air, the physical elevation, the cheerful society, and the like, to the salubrious influences of which

the patient is subjected; or else that some, as yet, undiscovered principle lies latent in the waters.

Under the impression that the latter might possibly be the case with those of Bath, I have lately concentrated by evaporation considerable quantities of this water, and have tested the residuum with the view of ascertaining whether, besides the ingredients determined by previous analysts to exist in it, certain other principles might not also be present, at least, in infinitesimal quantities. I was encouraged to make this attempt by the success obtained from their labours in that direction by Bunsen and Kirchoff, who, by evaporating about 12,000 gallons of the mineral water of Durkheim, in Bavaria, extracted from the solid residue two metals before unknown, which had first revealed their existence in the water to these chemists, by the application to them of their method of spectrum analysis.

I could, however, discover no trace either of fluorine, of barytes, of strontites, or of lithia in the solid constituents obtained from the Bath waters, although, with regard to the latter, I availed myself of the same delicate optical test, for which we are indebted to the above-named experimentalists, and by which, it is well known, the minute quantity present in the ashes of tobacco is readily detectable. The only new substances, of which I found reason to suspect the existence, were phosphoric acid and bromine; the latter indeed one which—from researches carried on by myself many years ago, on a number of mineral waters belonging to this county, as may be seen reported in my paper in the Philosophical Transactions for 1830 [d]—might have been conjectured as likely to exist, from finding iodine also in them, and indeed even from this element being so commonly associated with chloride of sodium, a salt which forms so large a part of the mineral constituents present in the Bath waters.

I do not indeed infer from these experiments the entire absence of the ingredients which I failed to detect,

[d] Reprinted in Part I. of this volume.

but only their not existing in quantities sufficient to communicate medicinal qualities to the water, for Professor Roscoe, whose skill and experience in the use of the spectrum apparatus is so well known, informs me, that he has detected lithia, barytes, and copper in these springs. To him, therefore, I must refer you for further information on this subject, merely remarking that these elements in minute quantity are so generally diffused throughout nature, that we should not be justified in attributing any medical efficacy to their presence, unless they had been found in larger quantities than those in which Professor Roscoe appears to have detected them.

Despairing, then, of being able to throw any additional light upon the cause of the efficacy of these springs in the cure of disorders, or of adding anything material to the information already supplied by competent authorities, as to the nature of the salts which impregnate them, I confined my attention to one point of scientific rather than of medical importance, namely, the quality and quantity of the gases which are disengaged through the same apertures or fissures in the earth from which the thermal springs themselves issue.

As the results of the examination I undertook for this purpose in the summer of 1832, are given in full detail in the "Philosophical Transactions," vol. cxxiv., I will not detain you further on this point than by stating, that the quantity of gas disengaged from the King's Bath oscillated between 530 and 80 cubic inches in a minute, averaging, however, in the course of the month during which the observations were continued, 267 cubic inches, and the usual range of variation being from 339 to 207. From these data, therefore, I inferred, that the quantity of gas issuing from this source alone could not be less than 222 cubic feet in 24 hours[c].

[c] I cannot reconcile the report made by Dr. Williamson as to the amount of gas given off in 1864, with that which I deduced from my own observations continued during an entire month in 1832, except by supposing that all the avenues of escape for the gas had not been so carefully stopped up at the time

Nor did the disengagement appear to be affected by the changes of atmospheric pressure, or other meteorological conditions which occurred during the period at which the observations were conducted. The phenomenon, too, is not of recent origin. We, at least, know that it was exhibited in full force a century and a half ago; for Guidot, who wrote on the Bath waters in 1696, seems to allude to it, when speaking of the manner in which they "bubble up, as if from a cauldron." Indeed, analogy would induce us to assign a much longer duration than this to the gaseous emanations of Bath.

We are assured that in other instances, where such phenomena present themselves in apparent connection with volcanic operations, no change has been noticed in their intensity from a period anterior to the Christian era. Thus the *Amsancti Valles*, situated in the Apennines, midway between the active volcano of Vesuvius and the extinct one of M. Vultur, were celebrated in the time of Virgil, on account of the same copious evolution of carbonic acid gas, which renders the access to them in certain spots perilous at the present time. The description given of them, and of the lake or pool which they contain, by the poet, fully bears out this inference.

The same remark applies to the phenomenon exhibited at the Lago Naftia, a pool or small lake situated about twenty miles from Catania, in Sicily. Here, as I myself witnessed, the water is in a state of continual ebullition, owing to the escape of gases, chiefly consisting of carbonic acid and sulphuretted hydrogen, accompanied with the vapour of petroleum, from which the name Naftia is derived. Now, it is well known from classical writers, that this very spot was from the same cause an object of popular

he made his observations, as they were during mine. Had it been a question of chemical analysis, I might have easily admitted that the mistake was upon my side, but when it was one of simple measurement, I can hardly imagine that so great a discrepancy could have occurred, if the evolution of gas continued as copious as it did thirty years ago, when, instead of 2,223 cubic millimetres (about equal to 87·5 cubic inches), the main quantity collected was not less than 267 cubic inches.

superstition in the early ages of Greece, and that an altar was there erected, upon which human sacrifices were at one time offered, dedicated to the Palici, the twin sons of Jupiter by the nymph Thalia, who, according to the fable narrated by Ovid, was concealed by the god from the vengeance of Juno by being buried underground, and who, when the time of her delivery was come, emerged from the earth with her two infants at the spot from which these mephitic gases proceed.

And even in places beyond the range of existing volcanic action, we appear to have evidence of gases being given out, at times as remote as those of ancient Greece, on the very spots at which travellers have recognised them at the present day. Thus Dr. Clarke, in his travels in Greece, tells us, that the hot springs which gush out from the foot of Mount Oeta, in Thessaly, at the pass of Thermopylæ, emit, at present, bubbles of sulphuretted hydrogen gas. Now this same spring is noticed by more than one of the writers of antiquity, and, according to Dr. Clarke, the allusion made by Sophocles, in his *Trachiniæ,* to the fable of the poisoned tunic which Deianira presented to Hercules, implies that the same evolution of gas took place from the spring at that period as at present. For the poet describes some of the wool of the garment, on being thrown down on the site of these hot waters, as causing frothy bubbles to rise from the earth.

From this and other facts of the same kind, which time alone prevents me from bringing forward, it may be inferred, reasoning from analogy, that the evolution of gas from the Bath waters is not due to any adventitious cause, but is essentially connected with the very existence of the heat which characterizes them; and this inference is confirmed by the peculiar nature of the gases which are here brought to the surface. Of these, a variable quantity, rising sometimes as high as 13 per cent. of the whole, but in general only amounting to 4·5, consisted of carbonic acid, the explanation of which I am disposed, for want of time, to reserve for another opportunity.

Confining myself, therefore, to the consideration of that

larger proportion, averaging about 95 or 96 per cent. of the whole quantity which rises from the spring, I may state that it is made up of the two constituents of the atmosphere, only in very different proportions from those in which they there exist, for, whereas in the air we breathe the oxygen bears the proportion of one-fifth to the nitrogen present, that in the gas exhaled from the Bath waters did not amount to more than four per cent., leaving for the nitrogen associated with it the whole remaining quantity.

Thus, the composition of the gas may be represented as follows: Carbonic acid, 4·5, oxygen, 3·8, nitrogen, 91·7 (95·5), total 100; whereas in common air there could have been present in the 95·5 parts: oxygen, 19·1, nitrogen, 76·4, total, 95·5[f]. If, therefore, the gas emitted be derived from atmospheric air, the latter must have parted with four-fifths of its oxygen before it reached the surface of the earth. One is also disposed to attach greater importance to this phenomenon, from finding that it is not limited to this one particular case, and cannot therefore be referred to any local peculiarity, but that it is common to all natural springs, whose temperature presents any marked excess over that belonging to the locality in which they present themselves[g].

At the time when I was carrying on the above inquiries, geologists were not so fully persuaded as they are at present

[f] From the analysis made of the gases by Dr. Williamson, and reported in the volume published by the British Association in 1865, it would appear, that I had over-estimated the proportion of oxygen, and therefore underrated that of nitrogen, which may very possibly have been the case, as the more accurate method of analysis by means of the pyrogallate of potass employed by him in some instances was not known in 1833, and that by explosion adopted in others could not be conveniently resorted to on the spot by myself. However this may be, the difference between the two analyses stands as follows:—

Mine in 1833.		Williamson's in 1865.	
Carbonic acid . . .	4·5	Carbonic acid . . .	3·138
Oxygen	3·8	Oxygen	·430
Nitrogen . . .	91·7	Nitrogen . . .	96·115
		Marsh gas . . .	·198
	100·0		99·881

[g] See List of Thermal Springs, p. 106.

with respect to the connection subsisting between thermal waters and volcanos; and I was, therefore, led to make this question the subject of a separate paper, which appeared in the "Edinburgh Philosophical Journal" in 1832. On this point, however, I shall not here dilate, as it is hardly likely to meet with opposition at the present day, and will merely observe, that if the almost constant disengagement of these gases from the interior of the globe, wherever thermal waters occur, is in itself a phenomenon of high interest—its importance is greatly enhanced—now that we are agreed to attribute it to the same great and widely-different agent to which volcanos themselves in all their various phases are referred. It is, therefore, rather to the bearings of this phenomenon upon the general theory of volcanos, than to the fact of its connection with that deeply-seated cause, that my present remarks are directed.

Now, if we are content to limit ourselves to the view taken by many geologists of the present day, and suppose all the phenomena that come under this category to be the simple results of the contraction of a cooling surface upon a molten interior, portions of which are pressed outwards, after certain intervals, at the points of least resistance, so as to give rise to ejections of lava, of scoriæ, and the like, it would be natural to expect, not only that the water which found its way to these depths would, in part, at least, be sent back to the surface in the form of steam, but likewise that it should be deprived by the heat of part, at least, of the air which it had previously held in solution.

Now the spring that supplies the King's and Queen's Baths, the most copious of any, discharges 128 gallons of water, or in round numbers (reckoning 277 cubic inches to the gallon) 34,900 inches per minute. Of this water 100 cubic inches were found by me to disengage, after long-continued boiling, $3\frac{1}{2}$ cubic inches of air, consisting of 2·9 cubic inches of carbonic acid, 0·4 of nitrogen, and 0·2 of oxygen, so that there would be present in 34,900 cubic inches, of carbonic acid, 1,012; nitrogen, 140; oxygen, 70; which quantity, added to that of the free gas disengaged per minute from the spring, would make up the

following amount, viz. carbonic acid present in the water, 1,012; disengaged or free, 12; total, 1,024: nitrogen, present in the water, 140; free, 245; total, 385: oxygen, present, 70; ditto, free, 10—80; total 1,489 cubic inches.

Now ordinary pump-water, taken from a calcareous district, such as that of Oxford, contains in 100 parts—Carbonic acid, 2·00; nitrogen, 0·90; oxygen, ·45; total, 3·35. So that 34,900 cubic inches of water would have been impregnated with, carbonic acid, 698; nitrogen, 314; oxygen, 167; total, 1,179 cubic inches. We find, therefore, that whilst the gas introduced into the interior of the earth by the water which penetrated it would amount only to 1,179 cubic inches, the aggregate amount of the same disengaged from the spring, or dissolved in it, would be 1,489, plus 304 cubic inches, an excess however due wholly to the carbonic acid and the nitrogen, for whilst the carbonic acid which entered the earth was 694, the carbonic acid emitted from it was 1,024, being an excess of 326; nitrogen entering the earth 314, nitrogen emitted 385, being an excess of 71; whereas the oxygen entering the earth was 167, oxygen emitted 80; shewing a deficiency of 87 oxygen, and an excess of 71 nitrogen, which latter is no doubt due to the quantity of air circulating through the earth, which would be deprived of its oxygen by processes going on in the interior.

Now, I am aware, it might be said, that the carbonic acid at all times disengaged by the mere action of heat upon calcareous strata, expelled the nitrogen which the water had contained by taking its place, and that we have the evidence of this displacement in the gaseous contents which the water presents upon analysis, as well as in the free carbonic acid emitted from the spring, as these together represent the quantity which the water at this temperature is unable to retain in solution. But when we examine the quality of the gases disengaged, we find such a solution inadmissible.

Ordinary water takes up one measure of oxygen to two of nitrogen. Now the 267 cubic inches disengaged from the spring consist of 95·5 per cent. of nitrogen and oxygen, in

the proportion of 3·8 of the one to 91·7 of the other, or 10·1 oxygen to 244·9 nitrogen, whereas this same quantity of air must have contained originally 85 oxygen, and 170 nitrogen.

If we turn to other thermal waters, it will be found that the quantities of gas evolved are very variable, although the quality is much the same; but as we possess, in few instances, any accurate information as to the amount of water issuing from the spring, and of the gas disengaged from the same source, the data are wanting for determining, whether or not the two bear the same relation one to the other in other cases, as they do at Bath. All we can say is, that in no known instance does the absolute quantity of nitrogen emitted approximate at all to what we observe in the case of the waters now under our consideration. This, however, may be owing to the air eliminated from the spring finding its way upwards in part through other channels than those through which the water escapes, and therefore eluding our means of observation.

But the most important point for us to consider is, that this same gas is emitted constantly from volcanos in their active as well as in their dormant condition. When I published my memoir on the Bath waters, in 1833, I was only aware of one case of the kind at Vesuvius—namely, that mentioned by Sir Humphry Davy, and of another noticed by his brother, Dr. John Davy, as occurring at the new volcanic island near Sicily, where nitrogen gas is stated to be disengaged in bubbles from the sea encircling that ephemeral product of submarine igneous action.

Since that time, however, evidences of the disengagement of nitrogen from volcanos have accumulated upon us from a variety of distinct quarters. Professor Bunsen of Heidelberg, has detected it issuing forth at various places in that volcanic island—both from fissures in the ground, and from the thermal springs which are so abundant. M. St. Claire Deville, too, in a series of memoirs published in the *Comptes Rendus*, of which an abstract is given in the *Annales de Chemie* for January, 1858, has pointed out the fact, that at Vesuvius, although wherever the fumaroles given off by the

apertures from whence the lavas issued are dry, or destitute of water, they consist, in general, of little else than atmospheric air; yet that when they evolve water, the accompanying gas is more or less deprived of the usual quantity of oxygen which accompanies nitrogen in the atmosphere.

The former of these facts may, I think, admit of a plausible explanation; but I will not stop to consider it, as the object of my present communication is simply to collect instances of the disengagement of nitrogen from the earth, accompanied with less than its usual complement of oxygen. And this Deville assures us is the case, not only at Vesuvius itself, but also amongst the dormant or extinct volcanos of the Solfatara and of the Lago d'Agnano; in several places likewise amongst the Lipari Islands; and in springs at the base of Mount Etna, although not from its crater.

The same fact may perhaps be likewise inferred indirectly from the occurrence of sal ammoniac amongst the volcanic products of Iceland, as well as in the lava of Vesuvius, and in the craters of the Solfatara and of Volcano. I think, therefore, there is ample reason for inferring, that the evolution of nitrogen gas, wholly or partially deprived of the normal proportion of oxygen accompanying it in the atmosphere, is essentially connected with that igneous action which is going on in the interior of the earth, and that it is met with in thermal waters, only because the latter derive their heat from the same chemical operations which give rise to the phenomena of volcanos.

And if this be the case, I do not see how any theory on the subject can be regarded as satisfactory, which does not embrace the explanation of a fact so remarkable. Let us, therefore, consider the various modes of accounting for it which have been suggested by different men of science.

Dr. Bischoff, the distinguished Professor of Bonn, who has devoted himself to the explanation of the chemical phenomena revealed to us by geology, suggests, that the extrication of nitrogen gas from volcanos and hot springs may be regarded, as simply due to the action of internal heat upon strata impregnated with organic matter. That nitrogen in small quantities should be contained in many rocks is in-

deed highly probable; both because those of aqueous deposition are constantly associated with the remains of plants and animals, which, during their decomposition, might be expected to disengage this element, and also because the attraction which all earthy matters exert for that gas would cause a portion at least of it to be arrested by the rocks through which it would have to pass at the moment of its evolution.

Nor need this be confined to rocks of aqueous formation, for even those of igneous origin might be expected to absorb from the atmosphere which pervades them some portion of the nitrogen that atmosphere contains. M. Delesse, indeed, by heating various rocks in contact with caustic potass, has succeeded in disengaging from most of them ammonia, thus proving, indirectly, in them the existence of nitrogen. He has even presented us with a table, in which is noted down the relative proportion of azote present, first in a recent bone, where it amounted to 5·1 per cent.; 2ndly, in one from Thebes in Egypt, where its exposure to the air had reduced its per-centage to 3·39; 3rdly, in one from a pumiceous conglomerate in the Brazils, where it amounted only to 1·69; 4thly, in vegetable mould, and in bituminous schist, in the former of which it rose as high as 2·0 per cent., and in the latter to 1·8; and, 5thly, in various rocks and minerals, in which the per-centage of azote ranged between 1·02 in the marl of Meudon, and 0·14 in the cellular lava of the Isle of Bourbon.

But in the first place, the quantity of azote locked up in this manner in the interior of the earth, contiguous to the site of the volcanic action, would seem insufficient to supply for an indefinite period that large and unintermitting flow of gas which we have seen to be in some localities taking place from the surface. Secondly, its emission, if due to such a cause, ought to be more general, instead of being confined to particular localities, and those, apparently, the ones affected by volcanic disturbances. Thirdly, if proceeding, as Bischof conjectured, from bituminous strata, it ought to be accompanied with those other gaseous and volatile matters which are the products of

the distillation of coal, and which actually do present themselves in those cases, where carbonaceous matters are brought under the influence of volcanic heat.

The eruption of Vesuvius, in 1861, seems to me to afford an illustration of what really happens when the internal heat of the globe operates upon strata containing bituminous matter in connection with calcareous strata[h]; and the same predominance of carbonic acid over nitrogen would probably be observed in the gases evolved at Bath, if either organic matter existing in the strata below, or nitrogen held in a condensed state within the pores of the rock, subjected to the internal heat, had occasioned this phenomenon, since the same force which disengaged the latter, would prove equally efficient in expelling at the same time the infinitely larger quantity of carbonic acid, which exists as a constituent of all limestones.

But, as we have seen, the relations between the two gases are exactly reversed—the carbonic acid bearing to the nitrogen the proportion of only about 4 to 91—so that the explanation would fail us most completely where the phenomenon is best exhibited—affording, perhaps, something like a clue to the comparatively trifling emissions of nitrogen that accompany volcanos, where the rocks, being porous, might, though destitute of bituminous or organic materials, contain a small amount of azote, unaccompanied with hydro-carbons or with carbonates, but wholly inapplicable to such cases as the thermal spring of Bath, where the nitrogen, if pent up in the rock, would necessarily be contained in calcareous strata, which must yield up their carbonic acid in much larger proportions, at the bidding of that igneous agent, which was competent to disengage the nitrogen present.

Another indirect proof of the connexion between the presence of nitrogen and the existence of volcanic operations is, as I have already remarked, the exhalation of ammoniacal salts, such as sal-ammoniac, which is observed to take place during their continuance. This salt, which has been de-

[h] See the Memoir already printed.

tected in a few thermal springs, as by Longchamp, in those of the Pyrenees, and also at Warmbrun, in Silesia, is found very abundantly amongst the lavas of Vesuvius shortly after they have been erupted, as well as in the Iceland volcanos, as we learn on the authority of Bunsen.

The latter chemist attributes its occurrence to the flow of lava over vegetable turf, which he suggests might undergo a slow distillation, and thus yield up its nitrogen in the form of ammonia. The competency of this mode of accounting for the ammoniacal salts occurring in lava after their emission from the volcano, I shall forbear to discuss, until it can be shewn to be applicable to cases where such products have been found in the crater, or in other spots where vegetable matter clearly does not exist. At Vesuvius, instances are given where sal-ammoniac was detected so high up the mountain as to render this account of its origin inadmissible; but others of a more decided character are furnished by the Solfatara of Puzzuoli, and by the crater of the Island of Volcano, in both of which this salt has been detected, associated in the latter instance with boracic acid. I have therefore suggested, in a memoir read before the Geological Society, and published in their "Proceedings" for 1858, another mode of explaining its presence[1].

Whether therefore the existence of nitrogen in the interior of the earth be inferred from its emission in a gaseous form from thermal springs, as is so remarkably exhibited here at Bath, or be deduced indirectly from its exhibiting itself as a constituent of the ammoniacal salts present in the lavas or in the craters of volcanos, we feel equally a sense of the inadequacy of the explanations offered for its occurrence, and seem compelled to fall back upon the more simple hypothesis, which assumes that some process of oxidation is going on near the locality in which the phenomenon manifests itself, such as should bring about an absorption of the oxygen present both in the air and the water which penetrates to these depths.

[1] See the Memoir already printed in Part II. p. 43.

This general conclusion is, perhaps, the only one we can legitimately deduce from the data before us; but the process of oxidation once admitted, it is hardly possible to abstain from speculating, as to the nature of the substances which are concerned in it, and on that of the products resulting. Now one thing at least is certain, namely, that the substances undergoing oxidation are not of the same kind as those combustible bodies existing on the surface, which evince the most powerful affinity for oxygen, such as the carbonaceous matters which we use for fuel. These by their oxidation would give rise to a very different class of products, such, for instance, as are afforded during the burning of beds of coal, but never by genuine volcanos.

Nor could the smouldering kind of combustion set up by the slow access of air to the strata, kindled, as we may suppose, in the first instance, by the decomposition of beds of pyrites, or in any other manner, at all resemble, either in its intensity, or in the nature of its effects, the energetic but intermitting operations going on within the focus of a burning mountain.

On the other hand, the phenomena more nearly resemble those which would occur, if water and air were alternately brought into contact with metallic bases possessing a strong affinity for oxygen. The consequence of such substances being in each other's proximity would be primarily an evolution of large volumes of hydrogen from the water, and of nitrogen from the air—these being the residues of the two compounds after oxygen had been abstracted from both by the chemical action exerted.

Now how far does this inference accord with the phenomena presented? Not, it may be objected, to the extent required by our hypothesis, for during the more violent phases of volcanic action it is still a question, whether the light proceeding from the crater is caused by the inflammation of gaseous matter, or is merely due to the reflection of incandescent materials, such as the red-hot stones ejected. But we should recollect, that the hydrogen evolved, owing to the decomposition of the water in the interior of the earth, would not in all cases necessarily find its way to the surface.

The existence of sulphur in the neighbourhood of volcanos, however it may be explained, is an undisputed fact; and this substance, volatilized by the intense heat, would combine with any hydrogen, as well as with any oxygen, which might be present, producing with the former sulphuretted hydrogen, and with the latter sulphurous acid gas. That both these products are abundant in most phases of volcanic action is sufficiently well known; nor is it less notorious, that these two gases, when brought into contact, are mutually decomposed, the oxygen of the one uniting with the hydrogen of the other, and the sulphur from both being precipitated.

Hence the inflammable gas which reaches the surface may only represent the excess of hydrogen remaining, over and above that which had been converted back into water through its affinity for the oxygen present. Nor is that quantity inconsiderable, for, not to speak of the springs impregnated with sulphuretted hydrogen which occur in the vicinity of all volcanos, satisfactory evidence has been afforded, first by Pilla, secondly by Deville, and, lastly, by Bunsen, that this and other compounds of hydrogen are freely disengaged from volcanos; and it is not to be wondered at, if they do not always kindle, even when issuing from a volcanic crater, since it is well known that the presence of muriatic acid, one of the most constant gaseous emanations proceeding from this source, prevents them from taking fire.

I have now, perhaps, reached the point to which the facts in our possession strictly warrant me in conducting you, but having discharged this duty, I seem to have earned the right of indulging a little in theory, especially when all that is speculative in what I have to advance is immediately borrowed from those principles which are assumed by the advocates of the opposite hypothesis, when they deduce volcanic operations from the mere re-action of the crust of the earth upon its fluid contents.

For, granting with the latter, that the globe was once in a nebulous condition, granting that the particles composing

it became determined by gravitation towards some common centre, and granting that in so doing sufficient heat was elicited to retain all its constituents in a molten condition, it would seem to follow that down to a certain stage in the progress of the earth's consolidation, the elements composing it would be kept separate, since the strongest chemical affinities would be annulled by the repulsive power of so intense a heat.

And this influence, which seems in itself so probable, has been verified, so far as regards two bodies which possess the strongest affinities one for the other, namely oxygen and hydrogen, by the researches of Mr. Grove, who, in the Philosophical Transactions for 1846, has shewn, that the heat generated by a voltaic current is powerful enough to decompose water into its constituents, by setting up a repulsive action more than equivalent to the force of chemical affinity which tends to bring their particles into mutual connection.

It can hardly, therefore, be doubted that a heat sufficient to maintain the most difficultly fusible of the earth's constituents in a liquid, not to say a gaseous condition, would be amply sufficient to prevent their elements from entering into combination. Having, therefore, so long ago as the year 1848, suggested in my work on volcanos, that such was likely to be the consequence of the intense heat which is supposed to have existed in the earlier period of this planet's history, I was pleased with finding the other day, that a transcendental philosopher—and one, too, who adopts to the full the theory, that volcanic eruptions, with all their accompanying phenomena, such as disruptions of the strata, and irregularities in the earth's crust, are due to the progressive contraction of the earth's solid envelope upon its cooling and contracting nucleus,—maintains, nevertheless, on theoretical grounds, the principle, that the constituents of our globe were originally dissociated, or in a separate and uncombined condition.

Mr. Herbert Spencer, in his chapter on the Instability of the Homogeneous, remarks as follows:—"Without raising the question, whether, as some think, the so-called simple

substances are themselves compounded of unknown elements, it will suffice for the present purpose to shew, how, in place of that comparative homogeneity of the earth's crust, chemically considered, which must have existed when its temperature was high, there has arisen during its cooling an increasing chemical heterogeneity; each element or compound, being unable to maintain its homogeneity in presence of various surrounding affinities, having fallen into heterogeneous combinations. Let us contemplate this change in detail. There is every reason to believe, that at *an extreme heat the bodies we call elements cannot combine.* Even under such heat as can be generated artificially, some very strong affinities yield, and the great majority of chemical compounds are decomposed at much lower temperatures. Whence it seems not improbable, that when the earth was in its first state of incanderance, there were no chemical combinations at all."

These passages, taken from a work published in 1862, are introduced to shew, not that the distinguished writer had borrowed any suggestions from the book on volcanos, or from any other publication of mine in which this position is maintained, but that the same idea had struck other minds, and had been arrived at by more than one process of thought, and by separate lines of inquiry.

Let us, then, briefly consider what would be likely to happen when the gradually-decreasing temperature had sunk to that point at which the more intense chemical affinities would begin to prevail. In the earlier stages of this planet's progress towards its present state of consolidation, it is conceivable that all its constituents would be maintained by the heat in a gaseous condition, when, according to the laws of diffusion, all the elements present would continue in a state of intimate commixture. But, long before chemical action could be set up, the denser ones would have coalesced into a state at least of liquidity, and under this condition gravitation would cause some separation of the heavier and lighter particles into concentric zones. The inner zones would, therefore, be occupied by the metallic elements, the outer

ones by the gases composing our atmosphere, and by those of which water and hydrochloric acid are made up. These, re-acting upon each other, and on some of those alkaline and earthy bases which came within the range of their affinities, would generate the waters of the ocean, and the salts which it contains, whilst the redundant oxygen, intermingling with the nitrogen, would produce an atmosphere enveloping the whole. These chemical re-actions would commence at or near the surface, as this had been the first to cool to the requisite extent, but they would afterwards extend to the interior of the mass, and produce combinations between the bodies there existing;—namely, the metallic and non-metallic elements,—those of alkalies, earths, and metals on the one hand, and phosphorus, boron, sulphur, &c., on the other. These combinations would take place between the metallic bases, if oxygen and chlorine were absent, but with the latter in the first instance, wherever these gaseous elements presented themselves. Hence, as far into the interior of the globe as air and water could penetrate, the bodies above-mentioned would now exist in an oxidized condition, in the form of alkalies, earths, and metallic oxides—whilst below that point, there is reason to suppose they would remain either in their original simplicity, or combined one with the other, as metallic alloys, or sulphurets.

I ask you, therefore, merely to grant me the continuation, at the present time, of the chemical actions which in earlier periods of the earth's formation would be set up, if the principles generally laid down by geologists are to be received. And I am prepared to present you with an explanation on chemical principles of all the phenomena which observation has made known to us. To follow out this hypothesis, however, through all its details might exhaust your patience, and occupy more time than can be allotted to this Lecture. Referring you, therefore, to my "Treatise on Volcanos" for a further development of this theory[k], I will wind up this paper by alluding to a more practical sub-

[k] See Appendix to this Paper.

ject connected with the Bath waters, to which I wish to call the attention more especially of the residents in this city.

On visiting, some years ago, the town of Chaudesaigues, in the south of France, I was surprised at being told that the method of heating houses by means of pipes of hot water, which we claim as an invention of the last century, had been practised there from time immemorial, only that the supply of water was derived from the hot springs which burst up near the town, and which are conveyed in pipes through the houses along the principal streets, until the excess of their heat is expended in the transit. Some time ago I was reminded of this circumstance, by what I heard of the advantage derived from the application of bottom-heat to horticultural purposes, of which a very striking example was afforded in the flowering of the *Bougainvillia spectabilis*, a Brazilian creeper, much admired in warmer countries, where it puts forth a profusion of gorgeous and luxuriant clusters of pendent flowers, but rarely producing blossoms in our stoves; so that the success which had attended the plan adopted by Mr. Daniels, the gardener to the Rev. Mr. Keene, of Swincomb House, in Oxfordshire, who had placed the roots in close proximity to a flue, so as to impart to them an unusual amount of heat, excited, a year or two ago, a good deal of interest in the neighbourhood. I was thus led to reflect upon the loss sustained at Bath by throwing away that large surplus quantity of heat which remains in the waters of the Thermal Springs, after they have supplied the various baths, and are allowed to drain into the river. Supposing the temperature at which the water issues from the baths to exceed that of the locality by only forty degrees (and I believe it will be found to be much more), we may easily imagine how great must be the waste of heat, when we reflect that in every twenty-four hours no less than 181,440 gallons of water are emitted from the King's Bath alone. Nothing, we would think, is easier than to arrest this stream of water in its passage towards the river, by causing it to flow through a coil of iron pipes let into the ground a few feet below the surface, so as to communicate

its heat to the soil within a given area, until it had itself sunk to the temperature of the place. Even without incurring any further expense than this, such an arrangement would secure to the plot of ground placed under the influence of this adventitious temperature a bottom heat sufficient for the growth of early vegetables, and for the cultivation of tender exotics. But if in addition to this a glass roof were provided, so as to cover over the area so heated, a winter garden might be obtained with scarcely any expense except the original outlay, by which the health and enjoyment of the inhabitants of this city might be largely promoted. The garden immediately contiguous to the Institution would seem, from its position with reference to the hot springs, especially adapted for such an undertaking, and from being sunk considerably below the street, would require a roof raised only a few feet above that level. I am told, however, that there exist certain legal difficulties which might prevent the application of the ground in question to such a purpose. Should these difficulties unfortunately prove insurmountable, I would then propose conveying the water, after it has done its duty at the baths, in pipes to the Sydney Gardens, and then causing it to circulate in the same manner through pipes inserted into the ground, until the whole of its surplus heat had become dissipated. As to the loss of heat which would be occasioned by its transit through underground pipes to such a distance some data are afforded by the elaborate report addressed in 1852 by my friend and colleague, Professor Maskelyne, to the President and Governors of the General Hospital at Bath, on the best means of transmission for the water to that locality along a distance computed at about twelve hundred yards. It appears from his account that the water, if conveyed in leaden pipes to such a distance, would not lose more than three degrees of temperature. Now the distance to the Sydney Gardens from the King's Bath may be reckoned at about 880 yards, and consequently the loss of temperature might not be more than two degrees during its passage thither. Moreover, the cooling influence of the surrounding medium would depend greatly upon the rapidity

with which the water was transmitted through it, so that the loss of heat would be diminished in proportion to the force of the mechanical agency by which it was propelled. It would seem, therefore, quite practicable to heat a considerable area, even at that distance from the spring, by the waters so conveyed, and if they were, in the first instance, emptied into a small pond, and afterwards transmitted from thence through the ground encircling it, by means of a coil of pipes, gradually embracing a larger circle as they extended, nothing but the protection of an external covering of glass would be required for the cultivation of the gigantic *Victoria Regia,* and other tropical water-lilies; whilst the borders of the pondwould at least secure to the inhabitants of Bath the enjoyment of many of the trees and shrubs of warmer countries in the open ground of the garden, and a participation in a genial atmosphere during the most rigorous seasons.

APPENDIX.

WITH reference to the hypothesis which I had advanced in the first edition of my "Description of Volcanos," published so long ago as 1826[1], and briefly adverted to in this Lecture, it may be worth while to point out that, whilst it has not, so far as I am aware, met up to the present time with any opposing facts, it seems to be confirmed by several particulars which the progress of discovery has since made known to us.

The first of these is the circumstance already alluded to, namely, the dissociation of certain elements, possessing for each other a strong affinity, under the influence of heat.

[1] This was eight years before Sir H. Delabeche (to whom M. Daubree, and, after him, Professor Warrington Smyth, in his Address to the Geological Society, 1867, appeal, as sanctioning, by his authority, the Chemical Theory of Volcanos which the French Philosopher had deduced from his Researches on Theoretical Geology, 1834, chapter vii.) pronounced it to be the one most consistent with the actual composition of the crust of the earth, and with the manner in which the water of the ocean might be regarded as the result of the combustion of hydrogen in contact with oxygen and chlorine.

This separation, first, I believe, pointed out by Mr. Grove as being brought about, with respect to oxygen and hydrogen, under the operation of electricity, has since been shewn to extend to the case of many other bodies, and to take place under a much smaller augmentation of heat; for Deville found that carbonic, sulphuric, and hydrochloric acids, were resolved into carbonic oxide, sulphurous acid, and chlorine by only a moderate temperature [m]. This view was lately brought prominently forward in a Lecture given at the Royal Institution, May 31, 1867, on "The Chemistry of the Primæval Earth," by Dr. Sterry Hunt, who does not appear to be aware that I had insisted upon the same fact in my work on "Volcanos," as well as in the preceding Memoir.

The second is, the now undeniable fact of the evolution from volcanos of inflammable gas, probably consisting of some combination of hydrogen, not only during their dormant, but also in their most active condition. Although it is not denied that incandescent lavas thrown into the air have been sometimes mistaken for flames, we have sufficient testimony from Bunsen in Iceland, from Pilla and others at Vesuvius, and from Deville in several localities, that gaseous matter is also evolved, and undergoes combustion.

Thirdly. The evolution of nitrogen, not only generally from thermal waters, but also from volcanos, as stated by Bunsen.

The fourth. The existence of iron in the state, not of a sesquioxide, but of a protoxide, in many volcanic materials, no doubt, through the imperfect oxidation of the mass, owing to the presence of hydrogen or sulphur.

The fifth. The researches of M. Daubree on meteorites, shewing the resemblance in composition between that class of bodies and eruptive rocks generally, may be appealed to, as affording a clue to the composition of the interior of the earth; and, if so, would lead us to infer, that the latter is made up of materials of a similar kind to those which exist in meteorites, and that the contact with oxidizing

[m] *Comptes Rendus*, vols. lix., lx.

agents, on or near the surface, has brought about those differences which distinguish chemically the known portions of the earth's crust, from the materials composing meteorites, and, therefore, as we assume, from the contents of the interior of our planet. These and other considerations of a similar nature lead M. Daubree to espouse the chemical theory of volcanos, which I have always advocated [n].

The sixth is the presence of water in all deeply-seated volcanic products in chemical combination, and of the same in those of a sub-aërial description held together by a sort of adhesive affinity.

The seventh is the detention of aqueous vapour, free muriatic acid and sal-ammoniac, within the pores and cavities of a newly-ejected stream of lava, for many months after its eruption, as was pointed out by myself in my "Description of the Eruption of Vesuvius," in 1834.

The Master of the Mint, in his late Paper on the "Occlusion of Hydrogen Gas by Meteoric Iron," published in the 33rd number of the Proceedings of the Royal Society, has shewn that hydrogen is in the same manner pent up within the cavities and pores of meteorites, thus affording another interesting analogy between them and the rocks composing the interior of the earth.

I do not know whether the fact pointed out in the second edition of my Descriptions of Volcanos, 1848, will be regarded by others, as it is by myself, more favourable to the chemical theory than to the mechanical one.

It indicates, at least, the gradual advance of chemical processes from lesser to greater depths in the interior of the earth. As silica possesses a lower specific gravity than the bases with which it is associated, it might be supposed to predominate in the more superficial portions of the crust, and, therefore, might form rocks containing a larger proportion of that ingredient.

I pointed out, in the work referred to, the gradual increase in the proportion of bases, as compared with that

[n] See Piazzi Smyth's Address to the Geological Society for 1867.

of silicic acid, which is observable as we ascend from the older igneous rocks to the products of modern volcanos. Thus granite is composed mainly of quartz, or uncombined silica, and of a felspar containing the largest amount of that body which can enter into combination with bases, namely, either orthoclase, adularia, or albite, which are trisilicates. Trachyte consists mainly of glassy felspar, also a trisilicate, but, as a rule, is destitute of that redundancy of silica, which exists in granite as quartz.

Lavas of an ordinary kind consist, of labradorite, in which the silica is only in the proportion of one atom instead of three, and of hornblende or augite, which consist of a single silicate of lime, soda, or protoxide of iron, combined with a double silicate of magnesia, protoxide of iron, or of manganese. In proceeding onwards, then, towards the more modern group of volcanic products, we find new ingredients successively coming into play; first, the alkalies increasing, then lime and magnesia becoming part of the constitution of the mineral mass; and, lastly, water entering into combination with the earthy materials.

Now this is in accordance with the lower specific gravity of silica as compared with alumina, lime, and other metals, such as manganese and iron, oxidised by igneous action, as may be seen by the following statement :—

	Specific gravity of
Silica	2·64
Lime	3·18
Alumina	3·95
Magnesia	3·60
Oxide of iron	5·00

Memoir on the Quantity and Quality of the Gases disengaged from the Thermal Spring which supplies the King's Bath in the City of Bath.

(*Philosophical Transactions, Vol. CXXIV. for 1834.*)

As these researches are noticed in the preceding Lecture on the Mineral Waters of Bath, I consider it unnecessary to reprint *in extenso* the Memoir refering to them.

List of Springs, &c., which evolve Nitrogen Gas.

The List which follows is extracted from various Memoirs, and Notes of observations of my own, and, therefore, rests upon my authority, except when the contrary is stated.

List of Springs, &c. which evolve Nitrogen Gas.

Name of the spring.	Locality.	Geological Position.	Temperature.	Gaseous Products evolved. Carbonic Acid.	Oxygen.	Nitrogen.	Authority.
Bath	Somersetshire	New red sandstone	115°	from 4 to 13	3·5	96·5	Daubeny.
Bristol	Gloucestershire	Carboniferous limestone	74°	3	8	92	Ditto.
Buxton	Derbyshire	Ditto	82°	0	...	Nearly 100	Pearson.
Bakewell	Ditto	Ditto	6 °	0	0	100	Daubeny.
Stoney Middleton	Ditto	Ditto	63°	0	0	100	Ditto.
Taafe's Well	Glamorganshire	Coal strata	70°	0	3·5	96·5	Ditto.
Mallow	County of Cork	Carboniferous limestone	72°	0	6·5	93.5	Ditto.
Holy Well	Near Clonmell	Ditto	Cold	...	6	94	Ditto.
Chandesaigues	Cantal, France	Gneiss	176°	No. 1· 57	13°	30	Ditto.
				—— 2. 60°	15°	25	Ditto.
				—— 3· 87	1°	12	Ditto.
Mont Dor	Auvergne	Volcanic rocks	113°	90	0·85	9·15	Ditto.
Vichy	Ditto	Tertiary rocks bordering on volcanos	113°	greater part	...	Traces	Longchamp.
Several	Pyrenees, France	Chiefly at the junction of stratified with intrusive rocks.—*Forbes*	Thermal of various temperatures	...	...	Nearly pure.	Longchamp.
St. Gervais	Savoy	Talc Slate	97°	...	...	Chiefly	Daubeny.
St. Marguerite	Valley of Cormayeur, Piedmont	Gneiss	64½°	0	...	Chiefly	Ditto.
Bonneval	Tarentaise	Clay state	...	12	...	88°	Ditto.
Loueche	Switzerland	Limestone	125½°	...	...	Nearly pure.	Ure.
Wiesbaden	Nassau	Chlorite slate	154°	73	0	27	Daubeny.
Wolkenstein	Saxony	Mica slate	83·5°	4	2	98	Ditto.
Wiesenbad	Ditto	Ditto	70°	8	2	98	Ditto.
Warmbrunn	Silesia	Granite	97°	0	5·3	94·7	Ditto.
Landeck	Ditto	Gneiss	85·5°	2	0	100	Ditto.
Borset	Near Aix-la-Chapelle	Junction of slate and carboniferous limestone	145°	18	2	80	Ditto.
Chaude Fontaine	Belgium	Coal formation	95°	...	...	Probably the greater proportion	Ditto.
Monte Catini	Tuscany	Apennine limestone	82°	10	4	98	Ditto.
Castellamare	Near Naples	Do., bordering on volcanic rocks	Cold	53	...	47	Ditto.
Torre del Annunciata	Ditto	Volcanic rocks	87°	predominating	16	84	Ditto.
Santa Lucia	Naples	Ditto	Cold	Ditto	14·5	85·5	Ditto.
Lago di Amsanto	Kingdom of Naples	Apennine limestone	Cold	Ditto	9	91	Ditto.
Aqua Santa	Mt. Vultur, Apulia	Volcanic rocks	Cold	Ditto	10	90	Ditto.
Lago di Solfatara	Tivoli, Roman territory	Ditto	Cold	Ditto	9·5	90·5	Ditto.
Water round the new volcanic island	Near Sicily	...	...	...	10	80	Dr. Davy.
Alhama	Andalusia	Secondary limestone	112·5	...	...	Nearly 100	Daubeny.
Warm Spring	Virginia	Formation 11 of Rogers*	97½ to 96	0.75	2·6	98·0	Ditto
Hot Spring	Ditto	Ditto	...	...	...		
Sweet Springs	Ditto	Ditto	108° to 93°	12·5 to 19·0	0 to 6	87·8 to 75·0	Ditto.
Snake River	Ditto	Ditto	...	...	...	72 to 67	Ditto.
Bath Springs	Ditto	Ditto	...	...	...	74 to 72·5	Ditto.
Lebanon	New York State	Talcose slate	73	...	10·6	89·4	Ditto.
Washita Springs	Arkansas State	Clay slate	148 to 118°	4	7·6	92·4	Ditto.

* Virginia and Pennsylvania Reports.

On the Occurrence of Phosphorite in Estremadura.

By Charles Daubeny, M.D., F.R.S., and Captain Widdrington, R.N.

(A Paper read before the Geological Society on Feb. 17, 1844.)

IT was generally believed in this country, on the concurrent testimony of most standard writers on mineralogy, that an extensive formation of phosphate of lime existed in certain parts of the Spanish province of Estremadura.

Such an opinion was calculated to excite a lively interest, not only in the minds of men of science, but likewise amongst practical agriculturists; for whilst the former would speculate as to the causes which could have brought together so large a deposit of a material, elsewhere found only in small crystals, except in connexion with animal matter, and would feel curious to ascertain whether the rock in which it occurred contained within itself any evidences of the existence of organic life, which might account for its formation, the latter would be desirous of learning, whether such a substance admitted of being employed in husbandry, as a substitute for bone-earth now so extensively applied as a manure, and, if it did, what might be the facilities for procuring it, and the means of its conveyance to the coast.

On inquiring into the sources from which the writers in question had drawn their information with respect to the existence of phosphorite in Spain, we soon became satisfied, that the prevalent notions on the subject might be traced altogether to some communications relative to it, which had appeared in a Spanish periodical, entitled, *Anales de Historia Natural*, published at Madrid about the commencement of the present century.

The first and most important of these is by the celebrated

chemist Proust, whose name would naturally pass in the world of science as a guarantee for any statement which he might put forth on his own authority.

His memoir, however, professes to be little more than the reprint of one existing in a periodical now difficult of access, entitled, *Anales del real Laboratorio de Quimica de Segovia*, which was published so long ago as the year 1788, and had also been translated in the *Journal de Physique* of Paris for the same year.

Nevertheless the re-insertion of the article by the same chemist in a journal of the year 1800, might be fairly regarded as an evidence that its author was still persuaded with regard to its general accuracy [a].

Proust begins by remarking, that the occurrence of phosphate of lime, forming *entire mountains* in Spain, furnishes a proof, that phosphoric acid is a substance not confined to the animal kingdom.

He then alludes to a notice, having reference to the same rock, which had appeared in the work, entitled, *Historia Natural de Espana*, by Bowles, in which we find the following passage :—

"From thence we proceed to Logrosan, a spot situated at the foot of a range of hills which runs from east to west, and goes by the name of the Mountains of Guadalupe. On leaving the said place, we meet with a vein of *phosphoric stone*, which crosses the Royal Road obliquely from north to south. This stone is of a pale colour, without taste, and, when scattered over live coals, emits a blue flame unattended with any smell."—(p. 60.)

It is remarkable, that this property of phosphorescing when heated, which first attracted attention, and had caused

[a] From information received more recently, and still more from an actual visit to the place, we became convinced, that Proust's speculations were either founded on the early account by Bowles, or had been taken up merely from an inspection of the specimens he had received from the spot, which he had never himself seen. Owing to his reputation throughout Europe as a first-rate practical chemist, more importance was attached to his accounts than they really deserve.

it to be commonly employed for that purpose in the neighbourhood to amuse children, though no proof in itself of the presence of phosphorus, being possessed by fluor-spar and many carbonates of lime totally destitute of all admixture of phosphoric acid, should have been the one which in the case before us led to the suspicion of its true chemical composition, for Bowles, though he speaks of the mineral as a *phosphoric stone,* seems to have affixed this name to it solely from an observation of this one character, and not from any further examination into its composition.

Though deficient, however, in this respect, the report given by Bowles is much more correct in the geological information it conveys, than the accounts which have subsequently appeared, and if it had been duly attended to, would have prevented a great deal of the misapprehension which has since prevailed as to the abundance and physical position of the mineral.

Indeed, it may be doubted, whether any one of the persons who have written on this rock subsequently to Bowles, were at Logrosan at all, their memoirs in general being mere paraphrases of, and speculations on, the facts given in the short description we have above translated.

After thus presenting us with the report of the mineral which had been furnished him by Bowles, Proust proceeds to describe, in the first place, its physical characters, and then the extraction of phosphorus directly from it, which is done by exposing it to heat in connexion with charcoal. There is also, in p. 138 of the same volume, a statement of an analysis of this stone made by Pelletier and Donadei, at Paris.

Proust then introduces a passage, which is probably the one that has given rise to all the exaggerated statements handed on from one writer to another respecting this mineral.

"The stone," he says, "occurs, not in veins, but in entire hills (*collados enteros*) in the vicinity of Logrosan, a village belonging to the jurisdiction of Truxillo, in the province of Estremadura. The houses and the walls which inclose the fields are built of it." To this, however, he appends a re-

mark, which, by shewing that his preceding statement was based on hearsay evidence only, ought to have suggested some caution as to the degree of credit which deserved to be attached to it.

"But," he says, "an actual inspection of the situation of these mountains, of their elevation, and their form, of their base, and the proportion they bear to those surrounding them, would have been more to the purpose than any conjectures that might be hazarded upon the subject. Not knowing, however, when I might have opportunity or time to survey them in person, I cannot at present undertake to give a more detailed account of their extent."

He then concludes with some speculations as to the origin of the phosphoric acid contained in the mineral, which it seems unnecessary here to repeat.

The second memoir in the same work relative to this stone is by Don Christiano Heergen, but being confined to an account of the external characters belonging to the mineral, it neither corrects nor confirms the preceding statement regarding its extent and geological relations.

Such, then, so far as we have been able to ascertain, was the amount of information to be derived from Spanish authorities, that gave rise to those statements respecting this mineral which have excited so much wonder and interest; and even at Madrid so little was known as to the real nature of the formation in which it occurs, that the first authority there, the head of the mining department, and himself a very able man, informed us that it constituted a vein (*filon*) in granite.

Hence all that we could learn respecting the rock in question was only calculated, from its very vagueness, to stimulate our curiosity, and to excite our imagination as to its real nature, and thus to create in us a mutual desire to visit in person a spot remarkable for the presence of so curious a substance.

And as it was intimated to us, that several leading members of the Royal English Agricultural Society entertained a wish to learn how far the site of the mineral in question

might be hereafter reckoned upon for furnishing our fields with phosphate of lime, should other sources of its supply fall off, we flattered ourselves that our researches might also prove of practical value, even if they should terminate merely in setting at rest a question which was exciting some interest at the time in quarters connected with agriculture.

Mr. Pusey, the late active President of that Society, entered warmly into our views, and through his kind intervention we obtained from the Right Hon. the Earl of Aberdeen, her Majesty's Secretary for Foreign Affairs, such letters, as were calculated to afford us the requisite means of exploring this remote and little visited province with comparative comfort and security.

We are also equally bound to acknowledge the unremitting kindness and attention shewn us during our stay in Spain, by our late Minister at the Court of Madrid, Sir Arthur Aston, by the Regent, and by the Spanish Government; indeed, our obligations are due to every individual, without exception, to whom we had occasion to apply, either for assistance or information, with reference to the object of our inquiries.

The phosphorite rock is correctly stated by Mr. Bowles to be situated at a short distance from Logrosan, which is a considerable village about seven Spanish leagues to the south-east of the town of Truxillo, in Estremadura.

It lies in that extensive clay-slate formation, which, with occasional masses of quartzite, constitutes the fundamental rock over a large portion of the country, from the time of our quitting the flat table-land of tertiary origin, which occupies the greater part of both the Castiles, till we descend the south-east escarpment of the Sierra Morena, and enter upon the plain of Andalusia.

We first met with rocks, which may, perhaps, be referable to this formation, near the village of Calzada de Oropesa, south of Talavera de la Reyna, and were led to conjecture that a change had taken place in the character of the substratum by the appearance of the country itself, which had

become more rocky, more diversified, and, at the same time, somewhat better clothed with wood [b] than before.

In the steep ravine through which the Tagus flows, near to the broken bridge of Almaraz, the rocks are observed to consist of a dark blue slate, and to be disposed in nearly vertical strata.

On ascending from thence to the Puerto de Miravete, the culminating point of this formation, such a bird's-eye view is obtained of the subjacent country to the south of us, as, by causing its minor inequalities of surface to disappear in the distance, is best calculated to convey to us a just general notion of its external configuration.

We perceive extending before us, so far as the eye can reach, a vast table-land, comparatively speaking, level; but on the one hand, dotted over at intervals with certain isolated and generally conical knolls, and on the other intersected by low ridges with flattened summits, rising to the height of 300 ft. or 400 ft. above the general level of the plain.

Of the former, the few which we either visited, or could obtain information concerning, seemed to consist of granite, which would appear to have forced its way through the midst of the slates in several places, but not, as represented in the map of the French geologist, Le Play [c], to constitute a continuous stratum over the country in which it at intervals is seen protruding.

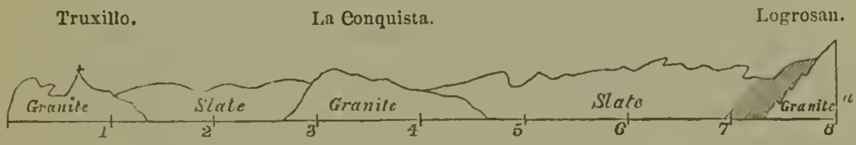
Thus granite forms the hill on which the old town of Truxillo is built, and extends for about a league into the plain on the road towards Logrosan, where it disappears under slates. The latter continue as far as a village distant about three leagues from Truxillo, which is built on granite.

We again find ourselves on this rock for about a mile, when it gives place to slates, which maintain their ground until we approach Logrosan. To the south of that village the granite a third time makes its appearance in a hill which rises to the height of 400 or 500 ft.; but, with this one

[b] Covered, however, chiefly by dwarf shrubs, amongst which the *Cistus ladaniferus* predominated.

[c] *Annales des Mines*, for 1836.

exception, the whole of the undulating surface of country which occurs round the town, and from thence to the Monastery of Guadalupe, consists either of clay-slate or of quartzite.



The granite is often so much acted upon by the weather, as to be separated into blocks, heaped one above the other, like the walls of some rude building of Cyclopean architecture; and to such an extent has the decomposition proceeded, that we are at first tempted to imagine that the blocks must have been transported from a distance, until reminded by their local occurrence, and their being piled up, so as to form considerable hills, that they are still *in situ.*

To the presence of the beds of quartz just before noticed may perhaps be attributed one feature in the physiognomy of the country we are describing, which has already been alluded to in this memoir, and which indeed has not escaped the attention of other geologists. We mean those low ridges of hills, with flattened summits, which here and there intersect the comparatively level surface of the clay-slate formation.

These summits are reported by Le Play to be commonly composed of quartz; and we are inclined to credit his statement, from what we remarked respecting the character of those above Almaden: nor does it seem improbable that the refractory nature of this rock may have enabled it to resist the action of those agents of decomposition that have worn down the surface of the slates contiguous, and thus to maintain more nearly its original level, just as in the Tyrol the incoherent materials, which have resulted from the *detritus*

a Phosphorite beds of Logrosan.*
Base, seven Spanish leagues.

* The slaty beds are grouped according to the following order:—
1. Dark blue, homogeneous, and excessively hard and compact fissile slate, intersected by veins of quartz. This is the common building-stone at Logrosan.
2. A soft and talcose slate.
3. A micaceous slate.
4. Alternating layers of talc and granular quartz.
5. Brecciated and slaty beds.

of older rocks, remain standing up to the height which they possessed when first deposited, wherever there happened to lie at the top of them a mass of stone large enough to shelter the parts underneath from the action of rain.

The beds of quartzite, which occur subordinate to, and interstratified with, the slate, present many varieties of structure, being sometimes compact, sometimes granular, possessing occasionally a brecciated character, and in other places hardly distinguishable from a fine-grained sand-stone.

The clay-slate itself, which constitutes the prevailing rock about Logrosan, is sometimes a dark blue homogeneous fissile schist; sometimes soft and talcose: it at other times contains scales of mica, and at others consists of alternating layers of compact felspar and of talc. Brecciated varieties also occur, which, but for their fissile character, we might refer to quartz rock; but, in fact, there is no decided line of demarcation between the two.

The most important question relative to this and its concomitant rocks relates to their age, and here the evidence is chiefly negative.

During our excursions about Logrosan, in the mountains of Guadalupe, and elsewhere, we met with no vestige of shells, or of other kinds of organic remains, nor could we learn that any such had been discovered in them.

Le Play, however, states that in the neighbourhood of Almaden slate rocks occur, in which shells are abundant; and he specifies the *Spirifer attenuatus* of Sowerby, and a *Terebratula* with small sides and large convolutions, as having been there met with.

We were also told, during our stay at Almaden, by some of the intelligent miners connected with that establishment, that Trilobites had been found in the same neighbourhood; so that no doubt need be entertained that rocks occur thereabouts which we may refer, as indeed the Spanish authorities at Madrid are disposed to do, to the Silurian epoch.

But whether the rocks surrounding Logrosan are precisely of the same age as about Almaden, or whether, as Le Play represents, those near the latter place consist of a more modern fossiliferous formation superimposed upon an ancient

one in which fossils have not yet been discovered, must remain undecided, until a sufficiently extended and minute survey has been made of the entire province, either to detect the presence of petrifications in them, or to justify us in pronouncing upon their absence.

In many cases, intervening between the granite and clay-slates, occurs a formation of a more crystalline character, which we found to assume the appearance of mica slate between Almaden and Cordova, near its contact with the granite of Viso, and which M. Le Play reports to be in other cases marked by the presence of crystals of chiastolite (*schist maclifere*).

It is in this clay-slate formation, which round about Logrosan seems remarkably compact, that the deposit of phosphorite occurs.

It may here be traced, either along the surface, or immediately underneath the soil, for a distance of nearly two miles, and in a direction from N.N.E. to S.S.W.; so that, if we may depend upon our own observations on this point, it runs conformably to what appears to be the direction of the rocks themselves about Logrosan.

Granting this to be the case, its position [d] would lead us to regard it as interstratified with clay-slate; and we shall therefore venture, in the remaining part of this memoir, thus to consider it, although aware that it has been spoken of by Le Play as intersecting the formation, and that the general direction assigned to the rocks of the country by that geologist is not in accordance with what appeared to us to belong to those immediately around Logrosan.

At its south-western extremity it seems to terminate not far from the base of the granite hill before noticed, but a little to the east of it.

This hill, indeed, on the side nearest to the phosphorite, consists of clay-slate, apparently heaved up by the granite, which constitutes its summit, as well as its north-eastern declivity; but the deposit in question has no connexion

[d] We say its position, because its mineral structure and general appearance certainly gave it very much the character of a vein, as will appear from the description given below.

with the latter rock, as was reported to us at Madrid to be the case.

At this, its south-western termination, the width of the deposit is nearly 16 ft. Of its depth we are unable to speak, as it has nowhere yet been fathomed to a greater extent than about 10 ft.; but it may be inferred, from its being found on the surface for nearly two miles, notwithstanding that the country traversed by it presents an undulating outline, with alternate elevations and depressions, amounting to at least 50 ft., that it penetrates to a considerable depth into the substance of the rock.

We traced the course of the mass from this point for about a quarter of a mile over a ploughed field, guided by the projecting portions of the mineral which protrude at intervals through the vegetable mould, or by the fragments of it which have been brought up by the plough.

We thus followed it up a gentle acclivity, and then again down a corresponding descent, till we reached the road leading from Logrosan to Guadalupe, where it was first noticed by Bowles.

Here, as the rock had been already worked, we had the best opportunity of examining it; and accordingly, although we caused excavations to be made in one or two other places, in order to satisfy ourselves that its general characters were the same throughout, yet our knowledge of it is principally derived from this one locality.

In this place it crosses the road obliquely from N.N.E. to S.S.W., interstratified with a rock consisting of a compact form of clay-slate, in which the slaty cleavage was indistinct, and which is disposed in almost vertical strata, inclining, however, from the granite of the adjoining hill, which has been already noticed.

The width of the phosphorite deposit at this spot is not more than 7 ft.; and it is to be remarked that, of this mass, the central portion only, to a thickness of about three feet, consists of phosphate of lime in a state approaching to purity.

The remainder is made up of alternate layers of hornstone and phosphorite, disposed in an agitiform manner

round certain nuclei of crystallization, the respective zones of the pure white variety being often separated, one from the other, by streaks or thin layers of the same, coloured with oxide of iron.

In consequence of the mode in which the materials filling up the fissure have been deposited round particular centres, to which they were most forcibly attracted, void spaces frequently exist between the several concretions which constitute the entire mass.

Where this has happened we often observe a mammillated structure, like that of certain chalcedonies, on the external surfaces of the mineral, with which mode of formation, indeed, the structure of the internal layers appeared on examination to accord.

Crystals of quartz are occasionally found coating the external layers, and likewise lining the walls of the cavities in them.

If we examine the particular structure of any one of those zones of phosphorite, which are wrapt round each other in the manner described, we shall find them often exhibiting a *stellated* arrangement, consisting of an assemblage of fibres or crystals radiating from a centre, as is the case in wavellite, for which mineral a small specimen of phosphorite might perhaps at first sight be mistaken.

The external characters of the mineral itself have already been sufficiently described in works on mineralogy.

The description given of it by Beudant defines in a few words, but with sufficient exactness, its peculiar structure, excepting that, misled by the erroneous notion as to its constituting "entire mountains," he represents as beds what are nothing more than layers or zones, running in a direction conformable to those of the phosphorite, and as veins, those which penetrate them transversely.

"The apatite of Estremadura," he says, "exists in fibrous, dendritic, stalactitic, testaceous deposits, either intermixed with quartzous beds, or intersected by veins of quartz, constituting entire hills near Truxillo, and employed as a building stone."

The inaccuracy of the former part of this concluding pas-

sage we have already sufficiently adverted to; the latter appears to have no better foundation than the use made of a few blocks of it close to the spot in which it occurs, in the construction of a wall which separates the road from an olive plantation contiguous.

The deposit, indeed, is traversed by the road, and must at one time have formed an inconvenient rise across it. It had consequently been blasted or broken down, and, as the fragments have been applied to patch the walls round about, we may account for the fable as to houses having been constructed of this material. At Logrosan the common building stone is the slate on which the town stands: it is dark blue, excessively hard and compact, with veins of quartz intersecting it.

The phosphorite differs, of course, in composition in different parts of the deposit; but no specimen which has been examined appears perfectly free from foreign matter, there being, in every instance, traces of iron, partly, at least, in the state of peroxide, together with one or two per cent. of silica, and a large proportion of fluoride of calcium.

The following are the results of an analysis made of one of the purest specimens that could be selected, or, we should rather say, the mean of two analyses, in either of which similar quantities taken from the same specimen were operated upon, the results of the two agreeing nearly together: —

Silica	1·70
Peroxide of iron	3·15
Fluoride of calcium	14·00
Phosphate of lime	81·15
	100·00

There was likewise 0·2 per cent. of chlorine, united with calcium, present in the mineral.

The silica and iron are probably accidental ingredients; but the fluoride of calcium is so commonly associated with

phosphate of lime, that it may be regarded as essential to the mineral in question.

It is known to occur, not only in mineral apatite, but also in a variable, but generally small proportion, in bones, and the enamel of teeth, both fossil and recent[e]; thus appearing to perform some hitherto unascertained office in the animal economy, so that a wonderful provision of nature seems to be unfolded to us, when we observe, treasured up in the older rocks, as if in anticipation of the wants of the future creation, a dépôt, not merely of an ingredient, like phosphate of lime, which was so requisite for building up all the solid parts of animals, but also, in constant association with it, a certain proportion of fluate of lime, which appears in like manner to enter into the composition of the same fabrics.

It will be remarked, however, that the per-centage of fluate of lime, which we have set down as existing in the mineral, is much larger than that present in any other specimen that had before been analysed.

Thus Gustavus Rose gives the analysis of seven varieties of apatite, taken from different localities, in which the largest proportion of fluoride of calcium discovered was 7·69 per cent., the smallest being only 4·59.

We should, therefore, have been reluctant in stating a result so little conformable with those of this distinguished chemist, had not there been a very near accordance between the results obtained from two specimens which were examined; and also if we had not considered that the varieties of apatite which Rose analyzed were probably all crystallized, whilst those of the Spanish phosphorite submitted to analysis were in a compact or amorphous condition.

The remarks just made have reference to the colourless or purer variety of the Spanish phosphorite, through which, as already observed, are disseminated streaks or thin zones

[e] See a paper of mine "On the occurrence of Fluorine in recent as well as in fossil Bones," in the Memoirs of the Chemical Society, Part IX., republished in Part I. of this Volume.

of a dark-brown variety. The latter was found to owe its colour to peroxide of iron, here present in a proportion varying from 15 to 20 per cent.; but neither manganese nor any other new substance could be discovered in it.

The quartzous veins, which alternate with the phosphorite, especially near the sides of the deposit, though consisting principally of silica, nevertheless contain iron, and a trace of phosphate of lime.

Other small seams of phosphorite proceed to some distance obliquely out of this, the main deposit, into the clay-slate rock; but none consisting of this mineral, unconnected with the one already mentioned, could we hear of, either in the neighbourhood of Logrosan, or in any other locality.

From the spot crossing the road above mentioned we traced the deposit in a S.S.W. direction across an olive plantation, down a gentle declivity, until we finally lost sight of it in the low ground beyond, about a mile from the road. Of course the phosphorite was only visible at intervals through the covering of soil; but the occurrence here and there of blocks or fragments of the mineral, and the conformity between the direction in which they appeared, and that of the previously observed portions of it, left no doubt in our minds as to this point.

The phosphorite throughout its whole extent is extremely indestructible, resisting the influence of the atmosphere, as well as of all the other agents of decomposition to which its loose fragments had been subjected.

In none of those which we saw, whether on the tops of walls, or partially buried under the earth, however long they may have been exposed to the weather, was the slightest change discoverable.

We were, therefore, anxious to ascertain, whether any influence had been exerted upon the suitableness of the soil for agricultural purposes by the presence in it of this mineral.

The soil itself bore the character of a loam, according to the definition given in the Table on the Classification of Soils, published by one of the authors of this memoir,

in Vol. iii. Part iii. of the Journal of the Royal English Agricultural Society.

1000 grains were separated by a mechanical analysis, conducted according to Mr. Rham's method, into—

	Grains.
1. Coarse pebbles	375
2. Minute do.	88
3. Coarse powder	388
4. Impalpable powder passing through the finest sieve	140
Loss being	9
	1000

Of the latter 100 grains were examined, and found to consist nearly as follows:

Of Water	5·0
Vegetable matter	4·5
Sand	50·0
Clay much charged with peroxide of iron	40·0

Carbonate of lime merely a trace.

Now a portion of this soil, after having been carefully freed from all the loose pebbles, &c., which it contained, being taken from the portion which had passed through the finest of the sieves, was carefully examined, in order to ascertain the presence in it of phosphate of lime, but only the merest trace of that substance could be discovered, less indeed than 1 grain in 2lbs. of the soil.

As this is not a chemical society, we refrain from entering upon the details of the method which was pursued, but believe that it would be regarded as conclusive, so far as relates to the detection of any considerable amount of this ingredient; and with regard to the small proportion which did exist, it might have been easily derived from the attrition of the fragments of phosphorite which were mixed

with the soil, as we had no means on the spot of sifting it with any accuracy.

It may, therefore, be fairly considered that the result of this examination confirms the conclusion with regard to the naturally slight tendency to decomposition in the mineral, which we had been led to entertain from our observation of it on the spot.

The soil brought home for examination was taken from various parts of a corn-field, immediately along the course of the deposit, which we were told was in itself by no means productive, but required to be improved by occasional top-dressings of stable-dung.

The presence of the phosphate does not, therefore, appear in this instance to communicate fertility; but the compact texture of the stone may perhaps render it in its natural state but little adapted for being secreted by plants.

It now only remains that we should state, in conclusion, the reasons which have induced us to occupy so much of the time of this Society in the description of the deposit alluded to. It has been conceived, then, to deserve a more particular notice.—In the first place, because it constitutes, we believe, a solitary instance of a rock of any magnitude or extent, as yet known, in which phosphate of lime occurs as the prevailing ingredient.

2ndly. Because the deposit not only contains in itself no traces of any organic matter, and possesses a crystalline structure entirely inconsistent with an organic origin, but also would seem, so far as our present knowledge extends, to lie in the midst of materials from which the evidences of animal life, if, indeed, they ever existed in them, have been entirely obliterated; for even if the fossiliferous slates of Almaden should be determined to belong to the same epoch with those about Logrosan, it must be recollected that they are at least fifty miles distant from the latter, whilst the existence of phosphate of lime in other instances, setting aside a few crystals of apatite occasionally met with in mineral veins, seems always referable to the presence either

of animal exuviæ, or of animal excretions in its immediate neighbourhood.

3rdly. On account of the exaggerated reports that had been given of its extent, which, though contradicted by M. Le Play, in the memoir already referred to, still held their ground in the public mind, as the cursory manner in which that geologist alludes to the phenomenon was so far from setting the question at rest, that it even left us in doubt whether he, any more than his predecessor Proust, had ever visited the locality.

The former, indeed, underrates the magnitude of the deposit, as much as the latter exaggerates it; for a rock varying from 7 to 16 feet in breadth, traceable for nearly two miles along the surface of the ground, and extending into the earth to a great, though as yet an unascertained depth, cannot be regarded as either unimportant or inconsiderable.

4thly. Because a statement of its chemical and mineralogical character, as well as of its relations to the rock in which it occurs, may lead to the discovery of it in other less distant and more accessible localities, and thus be the means of supplying a new source of phosphate of lime for agricultural purposes.

Lastly. We have been induced to enter more fully into its nature and relations in consequence of the interest which it has lately excited amongst agriculturists.

That it would prove equally efficacious as a manure with phosphate of lime derived from animal sources, must not, indeed, be taken for granted too readily, until this point has itself been made the subject of direct experiment; but there was a sufficient probability that such would prove the case, to induce us to take some trouble in ascertaining the price at which it might be procured, and the facilities that might offer for transporting it to our own country.

Were the Tagus now navigable up to Toledo, which, during the period when the crowns of Spain and Portugal were conjoined in the person of Philip II., it is said to have been the intention of the government to render it; two days' journey in the light carts of the country would convey the

material to the least distant point on that river, near the broken bridge of Almaraz.

But, at present, such is the retrograde state of things in the Peninsula, that the Tagus is not now navigable even so far as the frontiers of Portugal.

A still shorter distance separates the site of the phosphorite from the Guadiana, but much requires to be done before that river can be navigated even up to Badajoz.

At present the only practicable route by which to transport the mineral in question to the coast seems to be the one which we ourselves had recourse to, namely, that to Seville, a journey for mules of at least six days; this latter mode of conveyance, however, though the most convenient one for small quantities, is so expensive, as to be quite out of the question on the great scale, and the cheapest method would be that of resorting to the bullock cars of the country, which are extensively used in Estremadura, but only travel in troops together.

Should, however, the political difficulties, which the Portuguese government at present interposes to the transmission of goods by the Tagus, be hereafter removed, there seems no reason to doubt, but that it might descend that river at certain periods during the floods of the winter or of spring.

But although no prospect can be held out as to this mineral being profitably employed as a manure, unless the political condition of the Peninsula should become very different from what it is at present, and the expense of obtaining bones or other substitutes for them happen to be greatly augmented, still we do not regret having personally examined the locality, both because by so doing we have set, as it were, at rest, the question that had been mooted with respect to the uses to which this material might admit of being applied, and also because we have been enabled thereby to procure a quantity of it, sufficient for making trial of its virtues when applied to land capable of deriving benefit from bone manure; thus, as we hope, obtaining some data which may assist us hereafter in the determination of the interesting problem, as to whether the me-

chanical condition in which mineral substances are presented to the secreting organs, exercises any important influence upon their adaptation to supply the demands of the growing vegetable.

APPENDIX.

SINCE the preceding Report was published, mineral phosphate has been discovered in so many localities, that there seems no longer any reason to fear that the supply required for agricultural purposes will be exhausted. I may enumerate the following places from which I possess specimens in my own private Museum; and it would be easy to point out others not included in the list.

Phosphates may be divided into two classes, namely, first, those in which the mineral occurs as a vein, or as a bed, in a rock-formation, exhibiting no traces of organic matter; and, secondly, where it is disseminated through a rock in which traces of animal life are discoverable: the former apparently brought into the spot where we find it by chemical processes; the latter probably forming the residue of the bones and other parts of organisms in which it had existed as a constituent, and from which it had become separated when death and putrefaction had set in.

CLASS I.

Name.	Locality.	Description.
1. Phosphorite	Amberg, Bavaria	Resembling that of Estremadura, but containing some iodine.—(Voëlcker.)
2. Ostralite	Hanau. . . .	In thin seams in a volcanic rock; very rich in PO_5.
3. Apatite	Schlakenwald, Bohemia	In crystals.
4. Ditto	Kraageroe, Norway.	Contains about 35 to 40 per cent. of PO_5.—(Voëlcker.)
5. Ditto	New Jersey, in extensive beds	52 per cent. of phosphate.—(Way.)
6. Ditto	Jumello, near Alicant, Spain.	Contains from 74 to 85 per cent. of phosphate.
7. Phosphorite	Limburg, Duchy of Nassau.	The purer specimens contain about 34 per cent. of PO_5.—(Fresenius.)
8. Glaubapaeite and Pyroclasite	Mark's Island, Caribbean Sea.	Both minerals; containing a very large per-centage of PO_5.

CLASS II.

Name.	Locality.	Description.
1. Pseudocoprolites . . .	Felixstow, Suffolk.	From the Crag, consisting of nodules, containing from 44 to 36 per cent. of phosphates, and associated with abundant remains of modern animals, such as sharks' teeth, whales' ears, &c.
2. Phosphatic nodules . .	Cambridgeshire, Dorsetshire, Hampshire.	Occurring in the chalk, chiefly near its line of junction with green sand, containing from 54 to 58 per cent. of phosphates.—(Voëlcker.)
3. Phosphatic shale . . .	Near Oswestry .	Contains from 54 to 56 per cent. of PO_5.—(Voëlcker.)
4. Guano	Tropical Islands	The various deposits of this valuable manure, now so extensively employed in agriculture all over Europe and America, owe a large portion of their efficacy as fertilizers to the presence in them of phosphate of lime.
5. Rock Guano (so called) .	Sombrero Island, West Indies.	A guano, from which the nitrogenous ingredients have been removed by the action of water, containing 35 per cent. of PO_5.—(Voëlcker.)

Notwithstanding the ready means thus afforded of supplying our lands with phosphoric acid from various other sources, the richness of the phosphorite of Estremadura, which, as we have seen, contains 81 per cent. of phosphate of lime, has given it a preference over most other deposits, so that now that a railroad has been opened between Madrid and Lisbon, which passes within a short distance of Logrosan, a Phosphate of Lime Company has been started in London, intended to secure, for the purposes of agriculture, the Spanish as well as the Sombrero mineral; and although the former has been for the present abandoned, owing to some difficulties with the Proprietors of the Logrosan Mine, yet there can be no doubt but that sooner or later, now that the difficulties of transport are removed, the material will find its way into foreign markets, and serve the purposes for which it was shewn, so long ago as 1843, to be so remarkably adapted.

On the Use of the Spanish Phosphorite as a Manure.

(*From the Journal of the Royal Agricultural Society of England, Vol. VI., Part II., 1846.*)

IN 1845 a selection was made of thirteen different plots of ground in the Botanic Garden, Oxford, all of which might be regarded as in a great degree exhausted, having been cropped for ten or eleven successive years, without the application of any kind of manure, being the same upon which the experiments detailed in my Memoir "On the Rotation of Crops," published in the 135th volume of the "Philosophical Transactions," for 1845, had been instituted. The kind and quantity of the several manures employed are stated below, shewing that, whilst in every instance a considerable increase of crop was obtained by the addition of these fertilizers, the Spanish Phosphorite, especially when its action was quickened by the addition of sulphuric acid, proved nearly as efficacious as bones themselves, unless, indeed, when the latter were very finely powdered.

TURNIPS.

	Produce per Acre.					
	Roots.		Tops, including all the parts above ground.			Remarks.
1. Unmanured	14,298 lbs.		30,591 lbs.			Decaying, 2 lbs. dried by a water-bath weighed 1006 gr.: burnt. 101·5 gr.
Manured with		Gain.		Gain.	Loss.	
	lbs.	lbs.	lbs.	lbs.	lbs.	
2. Shavings of Bones, 10 cwt. to the Acre [a].	19,239	4,941	35,210	4,629	. .	Decaying and small.
3. Chemical Manure—Company's Guano, 260 lbs. to the Acre.	26,058	11,760	28,300	. .	2,291	Sound and tolerably equal, but smaller than those from Nos. 2, 6, and 7.
4. Nitrate of Soda, 1½ cwt. to the Acre.	28,459	14,161	45,302	14,711	. .	Sound, but rather small. 2 lbs. dried by a water-bath weighed 996 gr.; burnt, 124·5 gr.
5. Spanish Phosphorite, applied alone, 12 cwt. to the Acre.	28,639	14,341	42,016	11,425	. .	Sound and tolerably equal. 2 lbs. dried as above weighed 996 gr.; burnt, 103 gr.
6. Spanish Phosphorite, with Sulphuric Acid, 12 cwt. to the Acre.	30,869	16,571	34,476	13,879	. .	Sound and tolerably equal.
7. South American Guano, 260 lbs. to the Acre.	31,114	16,816	47,060	16,469	. .	Sound and tolerably equal. 2 lbs. dried as above weighed 1226 gr.; burnt, 95·5 gr.
8. Bones, with Sulphuric Acid, 11 cwt. to the Acre.	31,898	17,600	44,421	13,830	. .	Sound and tolerably equal.
9. Graham's Animal Compost, 260 lbs. to the Acre.	32,109	17,811	33,603	3,012	. .	Sound and tolerably equal.
10. Sulphate of Ammonia, 1 cwt. to the Acre.	32,670	18,372	46,464	15,873	. .	Sound, but of unequal size.
11. Bones finely powdered, 12 cwt. to the Acre.	36,185	21,887	45,446	14,855	. .	Sound and tolerably equal. Tubers rather larger than those from Nos. 5 and 6.
12. Potter's Guano, 260 lbs. to the Acre.	37,201	22,903	42,564	11,973	. .	Sound and tolerably equal. 2 lbs. dried as above, weighed 955 gr.; burnt, 96·5.
13. Stable Dung, 22 tons to the Acre.	39,476 [b]	25,178	49,912	19,321	. .	Sound, but unequal. 2 lbs. dried as above weighed 1010 gr.; burnt, 102 gr.

[a] The small increase of produce in this instance may perhaps be explained by the position of the bed, which was less favourably circumstanced with reference to sun and air than the remainder.

[b] The average of ten years' successive crops of turnips on the same plot of ground I find to have been about 16 tons to the acre. In my Memoir "On the Rotation of Crops" it is stated somewhat higher, owing to a mistake in the measurement of this plot, which I have discovered since the Paper went to press.

73